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TOWARDS A SCIENCE OF SOCIAL RELATIONS (I) *

G. A. Birks

1 *Object and Method*

Twenty years ago J. W. N. Sullivan, after surveying and comparing the achievements of scientists in all fields, came to the conclusion ' that it is only in the sciences dealing with inanimate matter that a reasonably adequate set of primary concepts has yet been isolated '.[1] Today most of us would agree that this is still true. Attempts have been made to fill the gap, but the concepts proposed by psychologists have little relation to the needs of the sociologists, those of sociologists have not helped psychologists, and neither have been adopted by historians. What Sullivan had in mind was no doubt something more ' primary ' than this, based on the recognition that all these specialists are concerned with human behaviour. In this essay an attempt will be made to explore the possibility of constructing a system of concepts adequate in the first place for a science of social relations, and ultimately for all studies of human life and activity.

The idea that inspires the undertaking is simple. In the old days of classical physics we used to be told that science is simply organised common sense, and, however inadequate this statement may be, it seems true enough that the Newtonian Laws do define with mathematical precision the common notion of matter as something that goes when and where it is pushed or pulled. Modern physics is diverging more widely, but could hardly have come into existence without classical physics. I see nothing inherently improbable in the suggestion that a science may develop through three stages, namely : (*a*) The loosely co-ordinated knowledge, acquired, transmitted, and tested over countless generations, which we call common sense ; (*b*) the systematic formulation of these common notions as a classical system covering and accounting for a large range of ordinary experiences ; and (*c*) further critical elaboration calling for new concepts.

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* Received 25. ii. 55

[1] J. W. N. Sullivan, *The Limitations of Science*, London, 1933, p. 201

Something of this kind has at least taken place in the two most exemplary sciences, geometry and physics. What I propose then is to clear the ground for a transition in the human sciences from the first stage to the second. Undoubtedly there already exists a fund of commonsense knowledge of human relations which serves us tolerably well in our everyday affairs, and has come under the scrutiny of historians and theologians, social reformers and legislators, as well as philosophers and others.

By 'common sense' I mean those beliefs which are implicit in our actions, whether we accept them in theory or not. For example, common sense rejects solipsism. This presupposition, obviously necessary for social sciences, marks at once a great cleavage from the physical sciences ; for classical mechanics admits no human agency or consciousness, and relativity physics postulates one observer only.

2 *Narrowing the Problem*

In social relations the units we are dealing with are individual men, women, and children, living in an environment of physical and biological objects. The possibility of a universal science of human behaviour depends in the first place on discovering universal human properties. Normal properties will not suffice, for abnormal people play a large part in human relations ; nor will average properties, such as those of economic man, for he is a fiction. The fact that no two human beings are alike does not mean that there are no universal properties ; the same is true of trees, yet these have well-known universal properties—growth, for example.

The commonsense distinction between a human being and any object that closely resembled it, such as a waxwork figure, would be based on behaviour. Anything that looked human and behaved intelligently would certainly be regarded as human. This indicates two things : first, that intelligent behaviour is accepted as universal among human beings, though no doubt at very different levels ; secondly, that it is too vaguely conceived to be applied as a sole criterion, without reference to outward form. It may be then that if we can sufficiently elucidate the conception of intelligent behaviour we shall find the concepts we are seeking. And this evidently means defining some constant relation between thought and action, however variable the thoughts and the actions of different people at different times may be.

I propose therefore to select a commonplace event as an example of human behaviour, and proceed by analysis. If there is indeed a constant relation to be found, it cannot matter what level of behaviour I take my example from. I therefore take it from the rational level, as being most familiar. My story is this :

Mr X is walking along a street intending to visit his friend Mr Y, whom he wishes to consult on business. Suddenly he catches sight of Mr Y walking towards him on the opposite side of the street. Mr X steps off the pavement to cross the road, and is knocked down by a fast car which he has not seen approaching.

I assume throughout that Mr X is fully aware of what he is doing and why he is doing it.

3 *Cause : Indeterminism*

If Mr X were asked why he stepped off the pavement, he might reply ' Because I recognised a friend I wished to speak to ', or ' In order to speak to a friend across the road '. One of these answers is a statement about cause, the other a statement about purpose, yet they mean the same thing. Evidently common sense accepts purpose as a cause of human action.

But purpose without situation does not explain action. If Mr Y had not been on the other side of the road, Mr X's desire to speak to him would not have caused him to cross. On the other hand, as soon as we know both situation (Mr X and Mr Y on opposite pavements) and purpose (Mr X's desire to speak to Mr Y) the action is completely intelligible, and we regard the action as fully explained. It is these two concepts, situation and purpose, that evidently need analysis.

Notice that common sense completely by-passes all mechanical explanations. An answer in terms of afferent and efferent nerves, or of ocular, cerebral, or muscular processes would not be accepted as an explanation of Mr X's crossing the road. This is perhaps comparable with the way classical mechanics by-passes what we are now led to conceive as to the complex molecular processes involved in the impact of two billiard balls.

But although situation and purpose explain action, they do not completely determine it. For if Mr X, instead of crossing the road, had stood still and beckoned to Mr Y, this course would equally be explained by the same situation and purpose. On reflection it is clear that, given a man in a particular situation, actuated by a particular purpose, there will in general be more than one course of action to achieve that purpose. Consistently with this view, historians are well aware that the most complete historical explanations do not account for the ensuing events to the exclusion of all other possibilities. One corollary of this is that no deterministic science can be built on the foundations we are investigating. Even if we could reduce the alternative courses of action by reference to character, habits, and predispositions, it is always possible that an element of pure chance may be encountered—as when I wish to cross a quadrangle from corner to corner without treading on the grass. We must then accept as a general principle that *the behaviour of individual men, women, and children is strictly indeterminate in terms of situation and purpose*, however these conceptions may be clarified. The example of modern physics suggests that this principle, which we may call the Principle of Free Will, is not an insuperable obstacle to the construction of a science.

4 *Situation*

We shall now attempt to elucidate the concept of situation.

It would probably be easy for the police to reconstruct from the reports of eye-witnesses the scene of Mr X's accident. A small map of the street might be made, and on this dotted lines would be drawn to indicate the movements of Mr X, Mr Y, and the car, up to the moment of impact. The situation thus depicted might well be called the objective situation—the situation as it would have appeared to the omniscient observer dear to classical physics. But it would be built up piecemeal from many fragmentary reports. The car driver would answer for his car, but did not see Mr X until just before he struck him, and probably knew nothing of Mr Y ; Mr X knew his own movements, and saw those of Mr Y, but did not see the car until it knocked him down ; Mr Y perhaps did not notice either Mr X or the car ; and so of other chance bystanders. It is very probable that no single witness would have as complete and precise a knowledge of the situation in its material aspects as the police later obtained. By the same argument, Collingwood has pointed out that a historian may often reconstruct a much fuller picture of a remote event than was attainable to those who took part in it.[1]

Certainly Mr X was not omniscient, or he would not have acted as he did. As long as we think only of the friend across the road to whom he wished to speak, and ignore the car, Mr X's action is quite intelligible ; it is the most obvious course to achieve his purpose.

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[1] R. G. Collingwood, *The Idea of History*, Oxford, 1946, p. 238

But as soon as we think of the whole or objective situation it becomes unintelligible, for stepping in front of a fast car would certainly be a most likely way of defeating his intention of speaking to his friend. Moreover, it is possible that Mr X's knowledge of the situation was positively erroneous as well as incomplete. He may have been mistaken in his recognition ; the person he saw may have been someone other than Mr Y. In this case too the action of crossing the road is intelligible if we think of the person as Mr Y (as Mr X did), but not if we think of him as a stranger to Mr X.

If then we are to understand human behaviour, we shall find it necessary to distinguish between the actual objective situation as known to an omniscient observer, and the incomplete or erroneous situation as it is known to the person whose behaviour we are studying. When we say that an action is explained or partly explained by reference to situation, clearly it is the latter situation, and not the former, that we must mean.

I therefore postulate a second general basic proposition : that *every man acts in a world which is known only to himself.* Or, since this means that all Mr X's thoughts and actions are concerned with this private world, we may say that every man lives in a world of his own. This we may call the Principle of Subjectivity.

Of course, that is not the end of the matter. In the sequel Mr X was struck by the car ; the objective world reacted on Mr X. But Mr X's part in this second event was passive ; and any such sequel is irrelevant as long as we are studying why Mr X stepped into the road.

Let it not be imagined that the Principle of Subjectivity commits us in any way to a subjective metaphysic. I am talking throughout on an empirical level, making the same naïvely realistic assumptions as have been made in the early stages of other sciences. I have not questioned that the buildings, the persons, and other objects known to Mr X are known also to other observers, or that they have some kind of independent or external existence. What I am doing is of course to carry one stage further the investigation into the relation between thought and action in human behaviour. After this explanation I shall not hesitate to speak of the subjective, as opposed to the objective, situation.

5 *The Subjective Situation*

Let us now examine more closely the world in which Mr X is living at the time of the story. We can regard Mr X's purpose as remaining constant throughout—that of speaking to Mr Y, and so set it completely aside for the moment. We shall find on reflection that we have not one world, but three worlds, to consider during the course of the story.

(1) *Before recognising Mr Y.* In this first stage Mr X is acting on the assumption that Mr Y is at home. His map of the situation would show the street, his own house, and that of Mr Y, and an unbroken line from one to the other indicating his line of motion (achieved or intended), together with a cross somewhere within Mr Y's house labelled ' Probable approximate position of Mr Y '. Only in such a world is his action of walking along the street intelligible.

(2) *After recognising Mr Y, but before being struck by the car.* In this second stage, there has been one change in Mr X's knowledge of the situation, involving two changes in his map. Instead of the cross marking Mr Y's position at home, there would be a dotted line along the opposite pavement marking his observed line of approach. And instead of his own dotted line continuing uninterrupted to Mr Y's house, it would end abruptly at a spot corresponding to a moment of uncertainty or irresolution, and then follow a new course across the road to intersect the line of his friend's progress. This again is the only world in which the action of crossing the road is intelligible.

(3) *After being struck by the car.* In this final stage Mr X has for the first time realised the existence of the car. He will know its direction of motion at the moment of impact, but will not be able to plot its line of approach with certainty. In all other relevant respects his map will resemble the police map—the objective situation.

Now the remarkable thing about these three worlds is that they are discontinuous. Mr Y disappears from the first, and pops up in quite a different place in the second ; in the third a car appears from nowhere. There is no such discontinuity in the objective world ; the police map would show all the movements of Mr X, Mr Y, and the car as unbroken lines, however irregular they might be. We are all quite familiar with this discontinuity of the subjective world, and we have a very common name to express it—surprise. Surprise is the emotional experience which accompanies the necessity of adjusting the world as we know it to the world as it is. Mr X would have been (perhaps mildly) surprised to see Mr Y in the street when he thought he was at home, and no doubt more surprised when the car struck him.

6 *Discontinuity*

This discontinuity is so remarkable—so different from anything in the physical world—that we must consider it more carefully. The Principle of Continuity has been one of the foundations on which science has been built, and although quantum physics has thrown some doubt on its validity at sub-atomic levels, it is still true that any discontinuity in the order of nature at the levels we are discussing would have to be regarded as miraculous.

First let us be clear as to what a physicist means by continuity. Imagine a pocketless billiard-table with a few balls on it, the table frictionless and the balls and cushions being perfectly elastic. Once set in motion, the balls would continue in motion indefinitely. The initial velocities could be measured, and defined in a set of equations. From time to time changes of motion would occur as ball met ball, or ball met cushion, but the new equations would be theoretically derivable from the old. In spite of the constant movement and the frequent changes of movement, such a system remains basically the same, endlessly and uninterruptedly evolving patterns which might have been foretold at the outset.

Now suppose that an inquisitive boy pokes one of the balls as it passes. The ball is deflected, and after a few collisions all the balls are affected. The whole world of the billiard balls has changed, and an entirely new set of equations, not derivable from the former, is required. There has been no marked discontinuity to the casual onlooker—no sudden appearance or disappearance of balls—and the deflection caused by the boy is no different in kind from those due to collisions. Yet to the mathematician the two systems are discontinuous. Without having seen the boy, he would know that some outside agency had tampered with the system.

Now there is something like this in the story of Mr X. I have said above that the objective situation is not discontinuous, but only in the sense that there are no such sudden vanishings and appearances as in the subjective situation. On closer examination it appears that while the movements of Mr Y and the car may be orderly and continuous enough, those of Mr X bear some resemblance to those of the ball poked by the boy's finger. At one period Mr X is walking along the pavement; then suddenly he is crossing the road. The mathematical equations have evidently been upset, for in the first system no accident was predictable, whereas in the second it was inevitable.

We need not jump to the conclusion that a miracle has occurred. Men are more complex than billiard balls, and if we knew more of the mechanics of human motion we might not find the question of the displacement of energy here involved to be insuperably difficult. But we are not concerned with mechanics further than this—that the two systems are unquestionably discontinuous in that they lead to different predictions.

I would next point out that this discontinuity in the objective world does not occur at the same time as the discontinuity in the subjective world. It occurs at the moment when Mr X steps off the pavement, whereas the subjective discontinuity occurs at two points, one just before this (when he recognises Mr Y) and the other just after (when struck by the car). It is true that Mr X seems to have been hasty and impulsive, and the interval between recognising his friend and stepping out may have been very small ; we can even conceive that the two acts might be simultaneous, though his behaviour then could hardly be described as rational. But a more cautious man might have stood on the kerb, looked right, looked left, and looked right again, and so increased the interval considerably. If simultaneity cannot be ruled out, it would be quite unwarrantable to assume it as a general, or even as a common, thing.

The search for discontinuity has thus enabled us to define three crucial moments in the story—the moment of recognition, the moment of stepping out, and the moment of impact with the car. Let us consider these more carefully.

7 *Creative Activity*

In what sense are these moments crucial ? We cannot say that Mr X is active at these moments and not at others ; for previous to the first moment he is walking, in the interval of uncertainty he will be satisfying himself that the man across the road really is Mr Y, and after stepping out he is again walking. Mr X is in fact active, either physically or mentally, throughout the story. The activities at the crucial moments are no different from other activities except in this —that they change the course of the story, or, in mathematicians' language, they lead to new predictions. In walking along the street Mr X is merely working out the implications of a previous decision ; in the interval of uncertainty he is working out in thought the implications of recognising Mr Y ; when walking across the road he is working out the implications of a second decision. During all these periods before, between, and after the crucial moments the system may be said to be static, in spite of the activities involved, and the story is seen to consist of static periods, punctuated by moments of discontinuity. If we can say that the boy created a new world within the borders of the billiard table, we can equally say that the activities of Mr X at the crucial moments created new worlds.

This phrase ' create new worlds ' is no mere figure of speech. Mr X's walking along the street is only intelligible in terms of the first world that existed in his mind, that in which Mr Y was at home. His crossing the street is only intelligible in another world, in which Mr Y is on the opposite pavement. If we are to understand his movements, these are very real worlds ; and since there is no continuity between them, but one vanishes and the other takes its place, the term ' create ' needs no apology. The second crucial moment is creative in a different way, for it creates no new world in Mr X's thoughts ; it is the car-driver that suddenly finds himself living in a new world. At one moment he is driving in a safe orderly world, with Mr X walking along the pavement ; at another he finds himself in a world in which disaster is imminent. It is Mr X's action in stepping off the pavement that has created this new world for the car driver. Here then is a clue to understanding at least one social relation. What I am doing is to substitute clear positive conceptions for the vague and often negative ones in common use. We might explain the accident by saying that Mr X did not notice the approaching car, and this would suffice in ordinary conversation as to this particular case. But who can disentangle clear concepts of general application from such a statement ? If on the other hand we bear in mind the world in which each person is living at each moment, and how the actions of one individual can change those worlds not only for himself but for other people, we are using perfectly general concepts which go far to explain both individual behaviour and social consequences. It so happens that the illustration I am using is a trivial incident from daily life. Let the reader think for a moment of more momentous single activities of single human beings—Newton's discovery of gravitation, or Faraday's invention of the dynamo, or Hitler's invasion of Poland—and he will have to admit that we are all quite literally living in new worlds in consequence.

It is of course inevitable that as elucidation proceeds our distinctions, though still based on common sense, must lead to notions and expressions increasingly unfamiliar.

I can now add a further general proposition, that *every human being is capable of activities which create anew the worlds in which related social activities are occurring.*

8 *Two Kinds of Creative Activity*

I have already distinguished between Mr X's three creative acts as of two kinds. When he recognises Mr Y, and again when he perceives the car, discontinuity occurs in the world in which he is living, the subjective world ; when he steps off the pavement, discontinuity occurs in the world in which the car driver is living, the objective world. This distinction is made, though in different terms, by common sense. We should commonly say that the former were acts of perception or recognition, affecting Mr X's knowledge, and that the latter was an act of will, affecting the relations between objects in the material world in a way that pure thought does not. The distinction however is not quite so simple as this.

We have not so far given much attention to the car driver. His world changes, strictly speaking, not when Mr X steps out, but when this change of course is perceived by the driver. An omniscient driver would have perceived it at once—and so would any other omniscient observer. It is therefore a discontinuity in the objective situation, like that of the billiard table. The discontinuity was in fact there to be observed by anyone, including Mr X himself, though it appears to have had less significance for others than for the driver. For him it would call for prompt action—pressing the horn, jamming on the brakes, trying to swerve clear—while for the casual bystander it would not matter at all.

The careful reader may have noticed that the course of the argument has led me to contradict myself. So far from wishing to cover this up, I draw attention to it as marking the point at which the common-sense distinction proves inadequate.

The contradiction is contained in the phrase above, 'including Mr X himself'. Up to this point I have submitted that the crucial moment when Mr X steps out is characterised by discontinuity in the objective world, but not in the world in which Mr X is living. It now appears that, since we have assumed at the outset that Mr X is aware of what he is doing, he cannot create discontinuity in the objective world without also creating discontinuity in his own subjective world—the same discontinuity that he creates for the car driver. This is obviously true of any act of will at the rational level ; it is not necessarily true of actions at other levels.

I must show before proceeding that this correction does not invalidate my former argument. When Mr X recognised Mr Y, his own world changed in such a way that his former course of action (walking to Mr Y's house) became unintelligible, and a new course (crossing the road) became intelligible ; the new situation called for new action. When he stepped off the pavement, in perceiving his own change of course he again experiences a discontinuity in the world in which he is living, from a world in which he is not crossing the road to one in which he is crossing the road. This change, however, does not call for new action, but only for a continuance of the action already initiated by the act of will. It is clear then that when discussing the intelligibility of Mr X's various actions we may legitimately ignore the change in the subjective world at the later of these two moments, while at the same time it may be of vital importance when we come to consider the nature of these activities.

Having found that the act of will involves subjective as well as objective discontinuity, we may suspect that something of the same sort may be true of the act of perception, but this proves to be only partly true. Certainly acts of thought on the rational level do involve acts of will—efforts of concentration, often if not always accompanied by physical actions or gestures. Thus Mr X, on catching sight of Mr Y, may have paused, turned his head in that direction, and focused his eyes. Such physical movements are certainly discontinuous in the objective world, just as much as stepping off the pavement. We cannot dismiss these as unimportant, for they might have caused quite a different ending to the story ; the sudden pause might have led to a collision with a pedestrian from behind, his turning the head might have led to a collision with a pedestrian in front, or either movement might have warned the driver of what to expect. As none of these things happened, we have legitimately ignored these gestures as negligible from the point of view of understanding Mr X's stepping off the pavement, but now we see that they substantiate the view that an act of thought at the rational level involves discontinuity in the objective as well as the subjective world, no less than the act of will. Yet there is a difference. We can conceive that the physical movements occurring as Mr X was walking might by a coincidence have brought his gaze into focus on Mr Y without any effort on his part, and recognition might have taken place at once. There is thus the possibility of a zero limit of objective discontinuity in the case of an act of thought, which has no counterpart in that of the act of will; for if a man changes his course of action, and knows that he does so, there must be positive discontinuity in both objective and subjective worlds.

Regarding this zero possibility as a limiting case, we conclude that every creative act is at the same time a creation of a new world for the person concerned and a creation of a new world for all other persons to perceive. If Mr X's stepping into the road had been simultaneous with his recognition of Mr Y (a possibility not entirely ruled out in the discussion in Section 6) we should have had an excellent illustration of this, with less trouble. The more general probability has needed a much longer analysis, but has been more illuminating.

The distinction between acts of thought and acts of will does not break down completely. The former call for new action, the latter do not. Again, in the act of recognition the discontinuity in Mr X's world (as to the location of Mr Y) has no resemblance to the discontinuity in the objective world (movements of head and eyes); while in the act of will they are the same (change of course). However, I am not concerned with these distinctions at the moment, but with the nature of the creative act in general.

My conclusion can be expressed in the following proposition: *Every creative act has two components,* (1) *a creation of a new subjective world for the person concerned*, and (2) *a creation of a new objective world*, though it is possible that the latter (only) in the limiting case may be zero. I am here using the term 'component' somewhat as the mathematician uses it. He speaks of the velocity of a projectile leaving a gun as having two components, one horizontal and the other vertical. These components do not exist independently, but they can be thought of and discussed separately; they are inseparable but logically distinct. I submit that in the same way the two components of the creative act are inseparable but logically distinct. They do not exist independently of the creative act or of one another, but can be logically distinguished and discussed separately, as in fact we have been doing throughout the last four sections. This is possible because, when we are discussing the behaviour of any Mr X, the terms subjective and objective form a perfect dichotomy, like the terms vertical and horizontal when referred to the plane of the projectile's trajectory. That is, they are mutually exclusive, and when abstracted from the creative act they leave no remainder. Otherwise nothing but confusion could result from discussing them independently.

(*To be concluded*)

ON EXTRAPOLATION, WITH SPECIAL REFERENCE TO THE 'AGE OF THE UNIVERSE'*

J. T. Davies

1 *Introduction*

The recently published essays[1] on the age of the universe, besides revealing the different meanings attached to the words 'age' and 'time', have thrown into relief the necessity for long extrapolations in both theories of the universe and in thermodynamics. In this paper I propose to examine some experimental results in which the validity of an extrapolation can be checked and which show under what conditions extrapolation is an approximation not inconsistent with the theory we are testing. While, strictly, no extrapolation can be logically sound, there are in our own experience, certain results which we extrapolate more confidently than others. I shall discuss here the question of when an extrapolation can be used as a good approximation in the absence of more detailed experimental data.

No certain formal criterion of the accuracy of an extrapolation is possible; without further experiment it is never known whether all variables have been taken into account. In spite of this, however, I shall show as an example that an extrapolation of the experimental results for a falling drop can be used to find the 'age' of the drop without appreciable error. More generally, under suitable conditions, extrapolation can provide a good approximation to the 'age' of any entity studied. The term 'age' is discussed in detail, and the analogy of the falling drop is finally used to illustrate the expressions 'the age of the universe' and 'the birth of the universe'. This meaning of 'the age of the universe' avoids any special philosophical difficulties: only the usual logical problems in any scientific methodology are encountered. Thus the age defined in this way is a strictly scientific concept, determined by normal scientific methods.

On the other hand, the time elapsed since a temporal origin or creation of the universe, i.e. the creation from nothing of space, time, and matter, cannot be determined scientifically.[1] It is in understanding such a temporal origin that interesting special philosophical problems of meaning arise. There are no scientific problems to be solved here ; suitable experiments, however impractical. cannot be suggested to measure such a time interval. The concept is not scientific.

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* This is an extended version of a paper presented before The Philosophy of Science Group on 24 January, 1955.

[1] 'The Age of the Universe', this *Journal*, 1954, 5, 181-252

Finally, the possibility of a ' winding-up ' of the universe is discussed on the basis of molecular probability theory.

2 *A Simple Physical Example of Extrapolation and of the Concept of Age*

To illustrate the meaning of the term ' age ', as well as the use of extrapolation, let us consider the following experiment. A little aniline is poured on to hot water. It lies on the water as an upper layer (Fig. 1*a*), but, as the system cools, this upper layer begins to bulge downwards (Figs. 1*b* and 1*c*). This is due to the cooling aniline becoming denser compared with water. When, on further cooling, the density of the aniline has slightly exceeded that of water, the bulge is more pronounced ; but still the aniline remains suspended above the water (as shown in Fig. 1*d*), held in the surface by forces of surface tension. When the temperature has fallen still further, and thus the density difference has become greater, the aniline eventually detaches itself from the water surface (Fig. 1*e*), and falls slowly as a large drop (Fig. 1*f*) to the bottom of the vessel.

If the displacement of the centre of gravity of the aniline below the surface is plotted against time one obtains the graph in Fig. 2. There is an initial region of slow, small displacements, followed by a second and linear region, corresponding respectively to the formation of the bulge and to the falling of the drop to the bottom of the vessel. The two regions of the graph are determined respectively by short-range forces (surface tensions) and by long-range forces (viscous drag in the water) ; this is of general validity and we shall discuss it further below.

What is the age of the drop of aniline in Fig. 1*f*? We do not mean the time elapsed since the aniline molecules were synthesised, nor do we call the bulge in the aniline layer in Fig. 1*a*, *b* and *c* the drop of aniline. Admittedly the pronounced bulge of aniline in Figs. 1*d* and 1*e* might be called a ' pendant drop ', but this leads to uncertainty in defining at what instant the bulge becomes a drop. I shall therefore talk of the ' age ' of the aniline drop as the time since the aniline

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[1] M. Scriven, ' The Age of the Universe ', this *Journal*, 1954, 5, 181

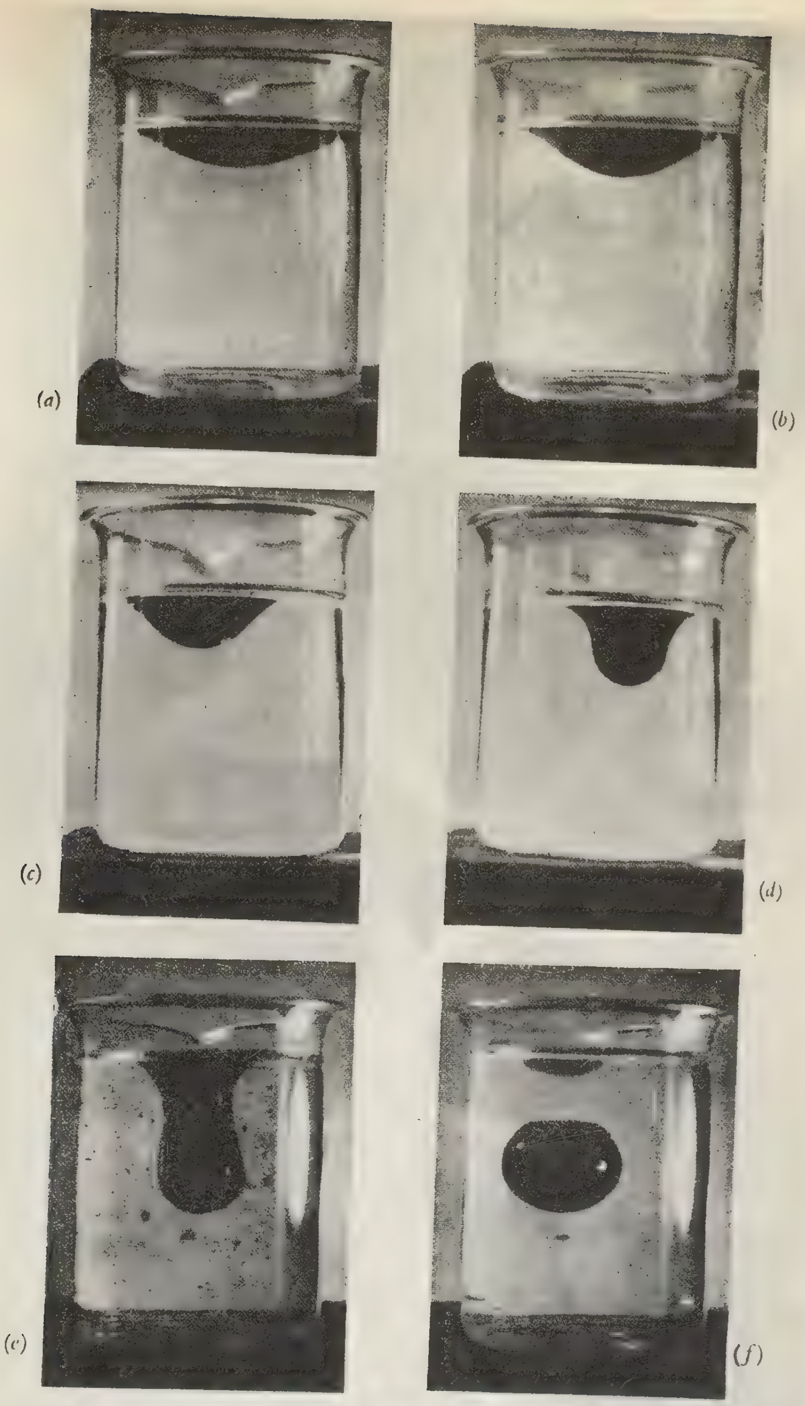

FIG. 1.—(*a*) Aniline overlying water at boiling point, time 11.45 a.m. ; (*b*) system cooler, time 11.57 a.m. ; (*c*) after further cooling, time 12.10 p.m. ; (*d*) bulge very pronounced at 12.33 p.m. ; (*e*) 'hour-glass' form, 12.39 p.m. ; (*f*) drop just formed and starting to fall slowly to bottom of vessel, 12.39¼ p.m.

broke away completely from the surface. Thus the ‘ birth ’ of the aniline drop is just prior to the moment at which the drop in Fig. 1*f* was photographed. If we had a picture of the ‘ waist ’ of Fig. 1*e* just broken, it would record the ‘ birth ’ of the aniline drop. Thus its age would be physically defined.

But if we were not quite quick enough to observe the exact moment when the ‘ waist ’ broke, could we still find the ‘ age ’ of the drop ? We can do so if we choose a suitable function (e.g. displacement of the aniline) and plot the available data as in Fig. 2 ; the ‘ birth ’ of the drop is marked by a sudden change in the form of the curve at the point ‘ *g* ’ (F_1, t_1) where surface forces are replaced by viscous resistance to the drop as it starts to fall. At this point the rate of change of gradient of the curve is a maximum, or, if F is the displacement and t is time, $d^3F/dt^3 = 0$. This is a general condition for a sharp break in a curve. Thus by examination of the data of Fig. 2 we can obtain t_1, the time at the birth of the aniline drop, even if we did not see the actual breaking of the ‘ waist ’. These two methods of determining the age will give almost identical results.

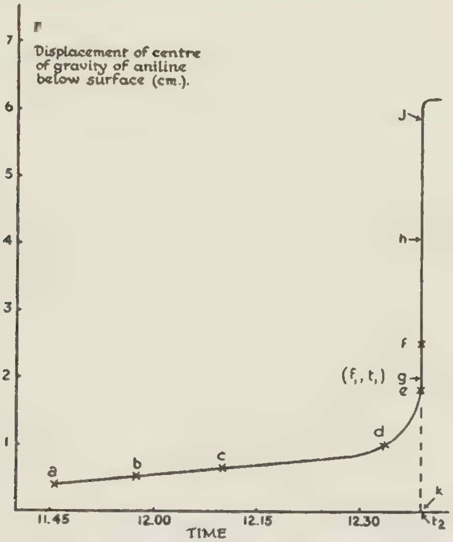

FIG. 2.—Plot of displacement (F) of centre of gravity of aniline below the air-liquid interface as a function of time (t). Data taken from Fig. 1.

Suppose now, however, that we did not see the early stages of the experiment at all, i.e. that Fig. 1*a*, *b*, *c*, *d*, and *e* were not available to us, but that pictures were taken only of the falling drop, as in Fig. 1*f* and also when it had fallen still further (points *h* and *j* of Fig. 2—actual pictures are not reproduced). Can we now obtain the approximate age of the drop? This is a direct analogy with what data we have of the expanding universe. We may extrapolate the experimental data on the falling drop (Fig. 2) back to zero *F*, corresponding to the point *k*. The time t_2 at this point will agree closely with the 'birth' of the drop at t_1 if certain conditions are satisfied.

3 *Conditions that Linear Extrapolation gives a good approximation to the time of 'birth' of the drop*

From what has been said above, the approximation here is that the time t_2 at the extrapolated point *k* is close to the time t_1 at the point where $d^3F/dt^3 = 0$. That is, the fraction $(F_1/(dF/dt)_{t>t_1})$ must be as small as possible if the extrapolated birth-time t_2 is to be in good accord with the time t_1 defined by $d^3F/dt^3 = 0$. This means that the value of *F* before the birth of the drop should be small, and that the slope afterwards should be large. These conditions are clearly satisfied in Fig. 2, and, as will be pointed out below, they are also satisfied as far as we know in determining the age of the universe by an analogous procedure.

The age is most clearly defined when the behaviour of the system before and after 'birth' is influenced by forces of different range, i.e. forces whose variation with distance is very different before and after the 'birth'. Thus the bulge of aniline in Fig. 1*d* is almost entirely a function of short-range forces (determining interfacial tension and varying approximately as the inverse sixth power of distance at the interface), while the falling drop (Fig. 1*f*) moves under the influence of long-range forces (flow of viscous liquids). In the intermediate stages (Fig. 1*e*) the form of the bulge is controlled by both types of force, but the viscous forces are still very much less important than they are after the 'birth' of the drop.

We can summarise the conditions under which the time t_2 at the extrapolated point *k* is a good approximation to the time t_1 at the kink in the curve (*g* in Fig. 2). Thus:

(i) *F* should be small at the kink point,
and (ii) dF/dt must be large after the kink point,
and (iii) the kink point should be clearly marked. This is so if $d^3F/dt^3 = 0$, though higher derivatives (e.g. d^4F/dt^4) equated to zero can also define the time of 'birth', if no lower derivative is zero.

All these conditions are likely to be fulfilled if there is a transition from control of the movement by short-range forces to control by long-range forces at the ' birth '.

We may see that this ' birth ' definition agrees with the use of the term ' birth ' as applied to an animal ; certain metabolic processes are controlled by short-range forces before birth (e.g. diffusion) and by longer-range processes (fluid flow of air and liquids) after birth. Thus if F is chosen to represent the flow of fluid in the lungs, the graph would again be similar to Fig. 2.

4 *Use of the Time of Birth*

If we have a theory about the values of F and dF/dt above the point g of Fig. 2, i.e. for the phase of rapid increase of F with time, this theory will predict the function θ in the expression :

$$F = \theta(t - t_1) \qquad \text{(i)}$$

Here t is the time at which any observation is made. If, as supposed above, it was not possible to measure t_1 directly, and if the three conditions above are satisfied, then we can approximate equation (i) by :

$$F = \theta(t - t_2) \qquad \text{(ii)}$$

Thus we can test the theory by comparing θ with experiment using the extrapolated time t_2 and equation (ii). For the aniline drop the theory is that of a drop falling in a viscous fluid (Stoke's Law).

5 *Philosophical Implications of Point* k *in Fig.* 2

Extrapolation to the point k ($F_2 = 0$, t_2) suggests physically that F was zero at time t_2, i.e. that the aniline occupied no volume at the ' birth ' time t_2, but then suddenly appeared in the system. This is, of course, unlikely and since in the aniline experiment we had obtained the points *a, b, c, d,* and *e* before t_2, we know the aniline did *not* appear suddenly from zero volume at time t_2. There is thus *no* physical meaning in extrapolating the data back to zero F ; it is merely a useful mathematical approximation for testing equation (i). Even if we had not obtained the points *a, b, c, d,* and *e* (having come into the room only just in time to record points *f, h* and *j*), we should not readily assume that the point k had any physical significance. We should, however, certainly take the simplest interpretation, i.e. that t_2 approximates to the birth time of the drop since we should have no better value (unless we had detailed theories of the short-range forces and constructed a theoretical curve over the region *abcdeg*). Thus the point *k* is a mathematical approximation only, and has no physical significance. It is logically consistent with a simple theory of the rapid change part of the curve, but not with a more complete theory covering the whole curve. In the absence of experimental evidence the point *k* serves only to define the approximate age $(t - t_2)$ to be used in equation (ii).

The Term 'Age'

The 'age' of the drop or animal is the time since it started behaving more or less as it was when we made our observations: the age of the drop is the time since it became detached from the surface and more or less spherical; the age of the animal is the time since it began eating and breathing as it does when we studied it. This is admittedly not rigorous; the mathematical definition of a break in the $F - t$ curve $(d^3F/dt^3 = 0)$ is more precise, and will usually approximate closely to the age as defined intuitively from appearance or behaviour. There is no mention of creation in these ideas of birth-time and age, nor in the approximation to t_1 of t_2 (Fig. 2). The point *k* implies neither creation from nothing nor an origin of time, but is merely a convenient approximation to the conditions at point *g*.

6 *Age of the Universe*

I have already shown[1] that by extrapolation of the red-shift data for the galaxies, the age of the universe is about 4×10^9 years. The assumptions, quite general in simple physical theories, are as follows[1]:

(i) the age is defined as in the previous section,
(ii) the red-shift can be interpreted as a Doppler effect,
(iii) the laws of physics are independent of the distribution of matter and of time,
(iv) the observations are precise in their simple forms, i.e. the red-shift is exactly proportional to distance,
(v) no new forces are operating.

From the intergalactic distances (F) and their rate of increase (dF/dt, from the red-shift), one draws straight lines for different galaxies corresponding to the line *fhj* of Fig. 2. With assumptions (i), (iii), (iv) and (v) above, one extrapolates the lines back to the point corresponding to *k*, i.e. to zero F which is now zero intergalactic distance. We have *no* information about any part of a curve *abcde*, but this does not mean that the point *k* implies physically either an origin of time at t_2 (4×10^9) years ago or a sudden creation of matter and space at that time.

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[1] J. T. Davies, 'The Age of the Universe', this *Journal*, 1954, **5**, 191

The time t_2 is merely the simplest approximation available for testing our ideas about the universe, and indeed the value of $(t - t_2)$, where t is the present time, has already proved of interest in relation to theories of the earth, the solar system, cosmic rays and the degree of spiralling of certain nebulae. This corresponds to testing Stokes' Law with the falling aniline drop. What happened to the galaxies before their 'birth' we do not know, but I do not believe it is impossible to find out. Theories of short-range nuclear repulsion might prove a guide, since to obtain the more complete curve (possibly of the type *abcdefghj*) one must substitute new information for assumption (v), in the region where F is small and short-range forces may become overwhelmingly important. This problem is purely scientific.

7 *Creation and Temporal Origin of the Universe*

If assumption (i) above is discarded, and we try to find the time interval since a creation from nothing of time, matter, and space, we must leave the field of science for theology or pure philosophy. Scientific method breaks down when the term 'nothing' enters the problem, and as Scriven[1] points out, science cannot even determine whether the universe has existed for a finite time or whether it has not. Discussion of these problems raises many interesting philosophical points, but there is no connection with the 'age' as defined in the previous section. That 'age' was a purely scientific approximation, but all we can say about the temporal origin of the earth is that if there was such an origin, it was more than 4×10^9 years ago.

8 *The 'Winding-Up' of the Universe*

According to the theory of molecular probability, occasional reversals of the Second Law of Thermodynamics should occur, with a decrease in entropy (a decrease in randomness) instead of the usual increase (which implies more randomness, or running down towards a final random state). Now such reversals should, on the simple theory of probability, be very infrequent, and though they have never been observed in macroscopic systems, these reversals have been suggested as a source of 'winding up' of the universe. For example, if on the earth all the molecules of water in a lake happened at some moment to be in the ice configuration the lake would freeze solid, and in that moment heat could flow into this sink from neighbouring bodies and energy could be obtained. Of course, the extrapolation of molecular probability to such large numbers of random molecules and to the time intervals calculated to be necessary for such a happening to be likely, is again a long one; and here I shall examine the basis of the extrapolation, and suggest that the lake will *never* freeze solid, any more than the monkey at the typewriter will ever type Shakespeare's sonnets. In both examples new factors may operate in the very long time intervals considered.

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[1] M. Scriven, 'The Age of the Universe', this *Journal*, 1954, 5, 181

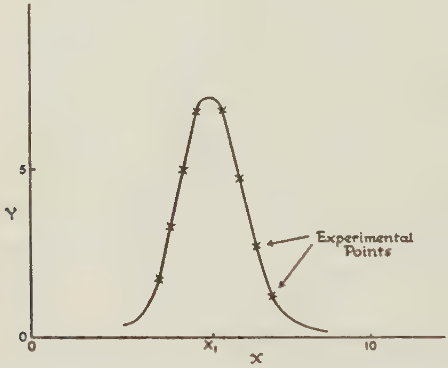

FIG. 3.—The normal curve of error, represented by equation (iii). Some typical experimental results are suggested.

As a concrete example, consider the normal curve of error,

$$y = A \, . \, e^{-b(x - x_1)^2} \qquad \text{(iii)}$$

To this simple mathematical function many observations can be fitted, at least in the region around the peak of the curve (Fig. 3): y could be the number of craters on the moon of depth x in a given area. Though, however the plot of equation (iii) might fit the data around the peak of the curve, we know that there are, in any area we select, certainly *no* craters as deep as (say) 10,000 miles. Thus while probability functions of the type of equation (iii) are useful in covering the data near $x = x_1$, the function cuts off in practice when $(x - x_1)$ is very large and y becomes zero, as it never does in equation (iii); extrapolation of equation (iii) to large values of $(x - x_1)$ may thus lead to absurd results.

Another example arises in the toxicity of reagents to living cells, or in haemolysis. In the latter, for example, y represents the number of cells of a resistance x to haemolysis, and here x is measured by the amount of haemolytic agent added. Such a distribution of resistances in a population is quite general, but members *never* have resistances greater or less than certain values.

These two examples show that extrapolation to very large values can involve absurdities if the extrapolated points are interpreted physically, just as the point 'k' of Fig. 2 had no physical meaning. I shall now suggest that the same sometimes applies in the interpretation of molecular probability theory. Let y in equation (iii) represent the probability of x molecules in a lake of water being simultaneously in the ice structure at some moment in the time interval Δt. If $y \,.\, \Delta t$ is constant, when y is very low we should, by equation (iii), expect to wait a long time for a large number of molecules simultaneously to be in the ice structure. I believe that, just as with the craters and the haemolysis of the blood cells, this is absurd : the lake will *never* become momentarily solid in the present conditions : the extrapolation of equation (iii) again has no physical significance, and macroscopic 'winding-up' never occurs.

9 *Modification of the Error function*

If equation (iii) is rewritten :

$$Z = (1/(x - x_1)^2)(ln(A/y)) = b \qquad . \quad . \quad \text{(iv)}$$

it is clear that when Z (which we may call the 'improbability factor' because if Z is large y becomes exceedingly small at large values of x) is plotted against x (Fig. 4), a straight line is obtained parallel to the x axis at a distance b above it. Now certain experimental results can be fitted on to the curve of equation (iv), usually around $x = x_1$; but when we apply equation (iv) to the craters or to the blood cells the improbability factor Z rises steeply to infinity at a certain value of $x(x_2)$ which represents the largest x value ever found, at which y becomes zero. This is shown in Fig. 5. Thus the extrapolation of the simplest curve (the straight line of Fig. 4) to very large values of x has no significance, and here it does not even furnish a useful approximation for testing theories. New factors have so modified the simple theory (equation (iii)) that Z must now be a function of x and not a constant (equation (iv) and Fig. 5), and the straight line is valid only over a limited range. To make y cut off to zero at x_2, we must modify equation (iii). For example, this may become:

$$y = A \, . \, e^{-b^1(x-x_1)^2} - \delta \qquad . \quad . \quad . \quad \text{(v)}$$

where δ is very small and related to x_2 by substituting $x = x_2$, $y = 0$ in equation (v). From equation (v), Z, defined as in the first part of equation (iv), is given by: $Z = b^1 + \dfrac{1}{(x-x_1)^2} \ln(1 + \dfrac{\delta}{y})$ (vi)

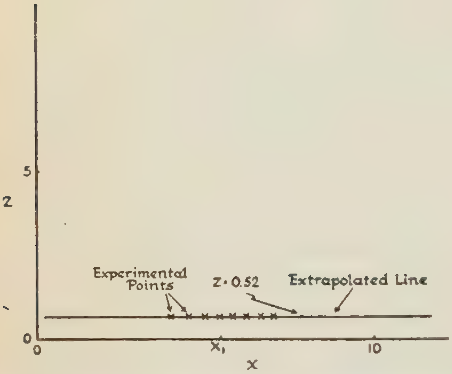

FIG. 4.—The simplest functions, as in equations (iii) and (iv), suggest that Z should be constant. This straight line is extrapolated in the figure.

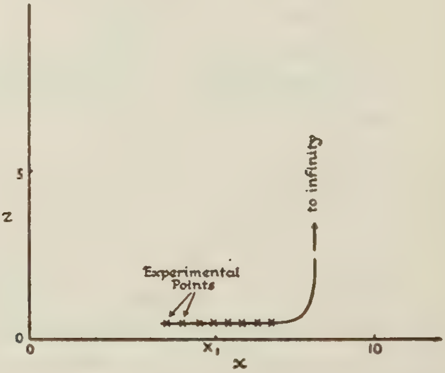

FIG. 5.—The modified equations (v) and (vi) (here $\delta = 0{\cdot}1$) allow y to become zero at a certain value of x. Thus Z rises to infinity at this point, actually confirmed from haemolysis and other experiments. This shows that extrapolation as in Fig. 4 can be unreliable.

This shows mathematically the behaviour in Fig. 5. The improbability factor Z is approximately constant only over a limited range—linear extrapolation is neither valid nor is it even a good approximation.

Thus equation (v) could cover the behaviour of blood cells in haemolysis, the depth of craters and, I suggest, also the molecules of water in a lake; for 10^{28} is a very large value of x with which to extrapolate the simplest rule (Fig. 4).

10 *Conclusion*

Extrapolation on the simplest possible basis, the straight line, gives a good idea of 'age' in certain systems. But extrapolations to very large values can often lead both to disagreement with experiment and to absurdities. There is absolutely no basis for suggesting that the universe could be 'wound-up' in accordance with molecular probability theory.

King's College
University of London

Department of Chemical Engineering
University of Cambridge

DO COMPUTERS THINK? (I)*

MARIO BUNGE

> . . . men say that the gods have a king, because they themselves are or were in ancient times under the rule of a king. For they imagine, not only the forms of the gods, but their ways of life to be like their own. Aristotle, *Politics*, 1252 b.

Introduction

THE simplest answers to the question whether computers think are, of course, the following : (*a*) No, machines do not and cannot think, because thinking is a spiritual activity, and spirit is totally alien to matter ; (*b*) yes, machines can think, as shown by the fact that modern digital computers are able to perform the highest mental operations, which are the mathematical ones.[1]

The first answer is based on the dualistic tenet ; to the upholders of this view, the claim that machines think is nothing short of a heresy. The second answer is usually not justified theoretically by those who utter it : they confine themselves to offer as a proof the obvious fact that computers perform complex operations which, when performed by human beings, are usually described as mental operations. Although no philosophical justification of this answer seems to have been advanced, it seems to be implicitly founded on the phenomenalist maxim that things are exactly as they appear to be, and on the pragmatist view that what counts are net results.

Those who do not believe that things are ultimately simple cannot endorse such answers. The dualist reply blocks every advance in the direction of building machines designed to save physical and mental toil and which, in an adequate social setting, would contribute to attain what Wiener[2] has called a human use of human beings. As to the behaviourist answer, it may be used and is actually being used for devaluing creative work in favour of the routine of ready-made operations ; besides, it does not foster the enquiry into the mental aspect of human life, as it usually takes for granted that no such mental

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* Received 16. xi. 54.

[1] See, for example, E. C. Berkeley, *Giant Brains*, New York, 1949

[2] N. Wiener, *The Human Use of Human Beings*, Boston, 1950

aspect exists ; finally, it brings us back to prescientific days, to the extent to which it assigns human attributes to inanimate objects.

I think that both solutions are dogmatic. The first, in so far as it assumes without proof the tenet of the absolute heterogeneity of substances ; the second, because it is an uncritical reply based on mere analogies. It seems necessary to try and look for a more satisfactory solution of this important problem, a solution based neither on the *a priori* rejection nor on the uncritical acceptance of phrases like 'The electronic brain will think for you', 'Computers may integrate differential equations', 'The reading machine is able to abstract', and other items of advertising. What I propose to do here in order to ascertain whether machines think or not, is to examine succinctly the two main aspects of the question, namely (*a*) the nature of computers, and (*b*) the nature of mathematical thought.

1 *Ideas and their Physical Marks*

Cyberneticians rightly claim that it is impossible to realise the scope of what they like to call the second industrial revolution while retaining the traditional idea that all machines are completely passive tools in the hands of craftsmen. They are right in maintaining that only an examination of artifacts endowed with a high degree of automatism can lead us to a reasonable evaluation of cybernetics. But it seems that they usually neglect the second side of the problem, viz. that of the nature of thought processes and objects, in particular of those which computers are said to handle. And, since machines are designed to mimic thought, a misunderstanding of the latter's nature will produce, by a sort of feed-back, a misunderstanding of the nature of the very machines designed by cyberneticians to replace some mental functions.

It is certainly true that, in so far as machines are the outcome of intelligent and purposive work, they cannot be put in the same class as natural inanimate objects ; machines are matter intelligently organised by technology, and as such they stand on a level of their own. But, on the other hand, it should be kept in mind that artifacts, however complex, operate only with material objects, never with ideal, abstract objects, a sort of operation which is precisely one of the distinctive characteristics of educated human beings. This elementary point is missed by most cyberneticians, and it seems to be the clue for the understanding of the whole question. Indeed, the confusion between 'thinking machines' and 'machines that replace thinking' lies in the identification of mathematical objects with their materialisations and, in general, in the identification of concepts and judgments with physical marks representing them. Once such an identity has been accepted, once such a confusion of the material and the ideal levels has been granted, it goes without saying that machines think.

We can express and record thoughts by material means, so that material objects (pennants, knots, acoustic or electric signals, spoken or written symbols, etc.) are correlated with them. Such physical marks correspond to thought, they stand for thoughts, they represent thoughts, they are deputies of thought—but they are not thoughts. If, instead of relatively static materialisations of concepts and ideas, we employ devices that combine and transform such physical marks (whether they do it automatically or not), then we will have constructed artifacts that do not think but which, up to a certain point, can represent and thus replace human thought within certain limits. All this is elementary, but seems to have been forgotten by cyberneticians, who systematically confuse ideal objects with their physical correlates.

I wish to emphasise the following points : (*a*) the physical processes organised by technology and involving material representatives of ideal objects, are *correlated* with reasoning, although they are not rational : what is at stake is not an identity in kind, but a similarity of pattern ; (*b*) those physical processes depend on the nature of the machine rather than on the nature of thought, as shown by the fact a given mental process can be materialised in several different forms ; that is to say, up to a certain point the physical marks used to represent ideas are contingent upon the latter's nature, and they do not depend on the context in which such ideas appear, since, when designing machines, we are interested only in external similarity, in resemblance of structure ; (*c*) such physical processes represent combinations of ready-made and clear-cut thoughts, but they are not able to create representatives of thoughts that do not arise as mere combinations of old thoughts in accordance with the rules of logic build in the machines.

All this is clearly illustrated by pencil and paper operations, for the problem of automatism, while central for technology, is rather irrelevant to the question whether machines think or not. Indeed, we are not asking whether machines think on immediate or on long-run command, but whether they think at all. We might as well take a step beyond, considering the first and simplest computer, viz. the abacus. But, since I do not wish to humiliate technicians, let me recall an artifact of a more evolved type, namely, Pascal's *machine arithmétique*

(1643), which is a sophisticated abacus.[1] In this artifact each integer, from 0 to 9, is represented by (or materialised in) a cog of a cogwheel ; there is one cogwheel for units, another for tens, and so on. When the first wheel turns one-tenth of a complete revolution, it has 'counted' 1 ; the gear wheels are connected in such a way that after 10 such unit rotations—i.e. after the machine has ' counted ' up to 10—the second wheel turns automatically one-tenth, thus recording 10 units in one stroke, while the first wheel regains the zero position—and so on. In this mechanism, which is substantially the same still used in desk-computers, the mathematical operation of addition is represented by the physical process of rotation of wheels through definite angles.

Computers differ according to the sort of material recording they employ. But, irrespective of their degree of automatism, they are all characterised by the fact that *they do not perform mathematical operations*, but only physical operations which we co-ordinate with mathematical operations. This holds not only for the computers of the analogue type (such as slide rules), but also for digital computers—in spite of the fact that it is usually stated that in the latter numbers are operated upon directly.[2] They all perform physical operations on entities (cogs, electric pulses, switches, etc.) that record ideal entities, that represent them at the level of technology ; in this respect, the essential difference between analogue and digital computers is that the latter, in contradistinction to the former, operate with numerable (discrete) events. Herein lies the decisive difference between natural inanimate objects and artifacts : the former are not the materialisation of images, concepts, ideas, etc., whereas artifacts *and other* concrete culture objects —such as books, paintings, etc.—do represent facts of mind in a material form.

2 *Counting*

The most advanced computers are at present those of the digital type, i.e. those based on co-ordinations of sequences of discrete material events (such as electric pulses). They perform operations which, when performed by man, are called arithmetical—i.e. addition, subtraction, multiplication, and division, as well as recognition of sign and equality of numbers. They also perform physical correlates of operations which, while not being arithmetical in kind, can be numerically approximated by sequences of arithmetical operations; for example, integration of most functions met with in practice may be numerically approximated, to any desired degree of accuracy, by summation.

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[1] Pascal, *Advertisement necessary to those who have curiosity to see the Arithmetic Machine, and to operate it*, in D. E. Smith (Ed.), *A Source Book in Mathematics*, New York and London, 1929

[2] See, for example, W. Sluckin, *Minds and Machines*, London, 1954, p. 13

By means of adequate devices, the four elementary operations can be reduced to addition; and addition of natural numbers and of ratios of them (which are the only ones recorded in digital machines) is after all reducible to counting, since any integer is obtained out of the preceding one by the operation $+ 1$, which is the typical operation performed in counting. Hence, in the last analysis, digital computers, working as they do with physical correlates of integers, are *counters at the physical level.* The basic traits of this kind of counting are: (*a*) it works on the basis of a co-ordination of *physical* entities; (*b*) computers are *specific*, that is, they do not count anything that is countable, but only very particular objects (angles, electric pulses, etc.).

Counting at the physical level is certainly analogous to the way primitive, or modern uneducated man, counts; indeed the latter use a correspondence between the things they want to count, and their fingers; i.e. they co-ordinate two sets consisting of material elements. But, whereas in the machine the co-ordination remains at the physical level, in man the connection between the two sets (e.g. shells and fingers) passes through a variable intelligence, and this enables man to count whatever he chooses to. But there is a higher level of counting; when mathematicians count, they usually operate neither like computers nor like uneducated men. What the mathematician means by counting is the process characterised by the following features: (*a*) to count is to establish a correspondence between the given set of objects and the set of *ideal* objects called natural numbers; this is what enables man (*b*) to count, at least in principle, every countable (discrete) set, whatever its nature may be; that is, mental counting is not specific, like physical counting, but *generic*; (*c*) besides, the mathematician knows not only how to count but also *what* counting is; for example, he is able to define counting as the co-ordination (one-to-one correspondence) between the given set, and the set of natural numbers; finally, (*d*), he knows why he is counting.

There is little room to doubt that machines do not attain this level of the operation of counting: they neither use abstract objects, nor are able to count everything, nor know why they work. Of course, for so-called practically-minded people such things are irrelevant, the important point being that machines count in some way. But this pragmatic attitude, besides being unphilosophical, may finish by blocking further technical advances in the building of servomechanisms, for it clearly entails the assumption that technology should tend to mimic mind at its lowest possible levels.

3 *Adding*

Machines add concrete numbers ; they do not add just numbers, i.e. pure, or abstract numbers ; hence, machines do not add in the usual sense of the word. In nature addition takes place in an infinity of concrete manners, as contrasted to arithmetics, where addition of numbers is univocally defined. Unlike nature, and unlike artifacts, man is able to build a mental level on the physiological one ; his cortex is able to do what no assemblage of valves, relays, and switches can do, viz. the feat of adding abstract, or pure, numbers, which need not refer to things outside them.

There can be no reasonable doubt that our brains perform abstract operations by means of very concrete physiological processes, just as physiological functions have in turn an underlying physico-chemical basis. Moreover, since in most cases our capacity to operate the abstract machinery is limited, we are forced to take pencil and paper, or other assistants—such as computers—in order to manipulate mental entities in a material form—a method which facilitates automatism and makes a larger use of the senses. That is, we handle ideal objects by means of material operations taking place within our skull, with or without the additional help of external materialisations of those ideal objects. In this way we are able, at least in principle, to add all imaginable things : not only angles but also angels, not only electric impulses but also emotional impulses. We are always able to translate concretely found numbers into abstract numbers, and vice versa. The ' nervous computing machine ', as Wiener calls our central nervous system, works not only at the physiological level but also at the logical level. This truism seems to have been forgotten by cyberneticians, who hold that computers operate with numbers,[1] i.e. with ideal objects, whereas on the other hand they claim that the operations of the mind are reducible to electric terms, so that brains work ' ultimately ' only at the physical level.[2]

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[1] W. Weiner, *Cybernetics*, New York and Paris, 1948, p. 136

[2] W. Grey Walter, 'An Electro-Mechanical " Animal " ', in M. Monnier (Ed.), *L'organisation des fonctions psychiques*, Neuchatel, 1951; W. R. Ashby, *Design for a Brain*, London, 1952

Machines, let us repeat, do not add pure numbers ; they add turns of cogwheels, electric pulses, etc., which combine in accordance with physical laws peculiar to each such material objects. Machines are not, as our nervous system is, multilevel structures ; they cannot retranslate concrete objects into their abstract correlates or vice versa. It is we who perform such a translation, when building and using the machine. We do it whenever we insert the input ' message ' (i.e. physical correlate of information plus operational symbols) and when we collect the output ' report '. In other words, the operator has to perform at least the following operations : (*a*) to translate a group of abstract (mathematical and/or logical) entities into the physical ' language ' of the computer ; (*b*) to retranslate the output ' message ' into the abstract language of mathematics and/or of logic. What the machine does in our place is the intermediary stage of ' information processing '. The amount and quality of mental work required to handle computers is such, that a specialist has recently written that, ' Perhaps, if IBM's familiar motto [" Think "] needs amending, it should be " Think : Think harder when you use the ' Ultimac ' " '.[1]

If one avoids using figurative language—and, particularly, if one avoids Wiener's mistake of assigning a language to machines—one is forced to admit that, strictly speaking, machines are neither fed with ' information ' nor ' work with logic ', nor ' report ' the results of their work, but that they are ' fed ' with physical marks (taping) and yield another group of physical marks of the same nature (output) which, when interpreted or read by the operator, get converted, in his brain, into authentic information.[2]

The foregoing applies to all of the operations performed by the computers of the digital type, since such operations are all reducible to counting and adding ; and most of our remarks apply also to other types of automata, for they all perform purely physical operations after a certain programme.

4 *Pythagorean Machines*

This is perhaps the place to point out an important limitation of digital computers, which cyberneticians, enthusiastic as they are over numerical results, tend to overlook : it is the fact that digital computers are utilisable only if the given problem is numerically reducible to a succession of arithmetical operations involving solely rational numbers. For example, they do not integrate, but add ; they do not yield square roots, but fractions approximating them. The differences between the obtained and the exact result may be negligible from the *quantitative* point of view, but it is enormous from a *qualitative* point of view. For instance, in the case of integration, an entity—infinity—and an operation—the approaching of the limit—are lost.

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[1] A. L. Samuel, *Proceedings of the Institute of Radio Engineers*, 1953, **41**, 1223

[2] The input taping and the output ' report ' may consist of a series of physical marks in a magnetic tape, or of holes on a punched card.

This shows the enormous limitation of digital machines : they are, to speak the pictorial language that cyberneticians love, Pythagorean machines, for they are limited to counting—at the physical level. They, not mathematics, fit into Mach's definition of the latter as ' the economy of counting '.[1] Since computers work with discrete strings of events, they ' ignore ' the continuum ; since they work with material objects, they ' ignore ' imaginary numbers ; and, working with actuals, they remain on this side of infinity. But irrational numbers, imaginary numbers, and infinity are just some of the most characteristic objects of classical mathematics, as contrasted with ancient mathematics. If mathematics is the science of infinity, as Weyl has claimed, then it is plain that computers, imprisoned as they are in the narrow frame of material representatives of natural finite numbers, do not perform mathematical work.

Practically-minded people tend to conceive of mathematics as The Art of Computing. No wonder, then, that they should believe that computers perform mathematical (and logical) work. Now, even granting that computers calculate (which, as we have seen, is not true), the truth is that computation does not exhaust mathematics. Computation is, to speak loosely, the ' mechanical ' side of mathematical work ; computation is a part of mathematics which is concerned neither with creating mathematical objects, nor with framing the rules in accordance to which they are operated upon, nor, of course, with their metamathematical examination. A computer, whether human or artificial, does not need to know the nature of the numbers with which he or it operates, nor to worry about the meaning and foundation of the rules governing the combinations of numbers : both numbers and rules are given to him or it, and he or it proceeds to use such a material in a blind, ' mechanical ', way. Computers, in sum, do not need to ' know ' what numbers are, but only how to work with their concrete correlates.

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[1] E. Mach, *The Science of Mechanics*, transl. by T. J. McCormack, La Salle (Ill.) and London, 1942, pp. 583, 584

This does not mean that machines are equivalent to human computers; although some net results of computing machines may be correlated with some net results obtained by human computers, the difference between them is as big as between a striped sweater and a zebra. For, whereas human computers know at least how numbers work—or at the very least how their written representatives work—computing machines do not *know* it—nor know anything else. In the first place, because they do not work with mathematical objects but with physical representatives of them. Secondly, because computing machines just perform certain operations without being aware of it: they do not know what they are doing nor even *that* they are doing anything—and this simply because machines have no consciousness, which is a prerequisite for the non-automatic type of knowledge.

5 *Are Machines Aware?*

I have just employed a word which is the *bête noire* of behaviourists and cyberneticians, namely, consciousness. The arguments employing this word will consequently not be accepted by them. Since this is not the place to defend knowing as the highest function of consciousness, nor consciousness as the highest level of the person, I will ask the reader to accept the fact of consciousness as a primitive in the philosophical context—though as a central problem of human psychology—and to receive, just to prevent misunderstandings, an admittedly coarse definition of knowledge. It runs as follows: Knowledge of X is awareness of the fact that X has such and such characteristics.

On this definition it is obviously wrong to hold that machines know anything, for they lack consciousness. To say that a machine knows how to solve the problem stated in the programme is like saying that the planets know the Keplerian laws which they approximately follow, or that plants know how to photosynthesise. On the other hand, our definition ensures that mathematics belongs to the field of knowledge, since it is practised by men aware of their own work—at least in the interesting cases. If mathematics had nothing to do with concepts and were nothing but the blind performance of non-conceptual operations, as Hegel[1] thought, then it would not be a branch of knowledge. The Hegelian Croce [2] would then be justified in asserting that mathematics—which he called the ape of philosophy, *simia philosophiae*—has no cognitive value, but only a practical value. And the cybernetician McCulloch [1] would also be justified in writing that ' We are about to conceive of the knower as a computing machine'.

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[1] Hegel, *Science of Logic*, transl. by W. H. Johnston and L. G. Struthers, London, 1929, Vol. 2, p. 322

[2] B. Croce, *Logica come scienza del concetto puro*, Bari, 1928, pp. 233-4 and *passim*.

Fortunately this is not so ; mathematics belong to the highest achievements of human knowledge, and in order to be a mathematician—i.e. a knower of and a worker in some branch of mathematics—it is not only required to know how to calculate something (in the large sense of the word calculation, i.e. meant as combination of propositions), but also to know what and why it is being done. That is, the theoretical value of mathematics is great because it is not reducible to a set of calculi.

A mere glance at actual mathematical work—not, however, at certain books on the philosophy of mathematics—will convince anyone that it takes place at various levels, no one of them being entirely reducible to the combinatory level, i.e. to calculation—which is precisely what computing and ' logical ' machines mimic. Besides inference of the analytic type, mathematics contains two further layers : (*a*) a *synthetic* level, consisting of the framing of definitions, postulates, hypotheses, rules of operation, etc.—in short, all the concepts and propositions belonging to the level of principles ; (*b*) a *critical* level, characterised by the critical examination of principles and theorems, and the clarification of their meaning—which may lead to the reconstruction of theories, i.e. to new stages of the synthetic level.

To use pictorial language, one may say that machines ' apply ' principles. But, so far as I know, nobody has as yet proposed to design a machine capable of building a new branch of mathematics, nor to make a criticism of whatever inconsistencies may be found in available mathematical theories—in spite of the fact that cyberneticians claim that the processes of criticism and consequent correction are always ' essentially ' of the feed-back type.

(*To be concluded*)

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[1] W. S. McCulloch, ' Through the Den of the Metaphysician ', this *Journal*, 1954, 5, 18

NOTES AND COMMENTS

A Reply to Professor Haldane

In the Preface to *The Concept of Nature* (1930) A. N. Whitehead speaks of his endeavours to 'prevent the reader from bolting up side tracks in pursuit of misunderstandings'. With the passing of the years, the growth of reading material and the introduction of new ideas and new techniques of expression, the prevention of misunderstandings becomes more and more difficult. The difficulty is not dependent only on the increasing specialisation of inquiry ; it partly depends on ourselves. When some new suggestion is made we can either take a sympathetic attitude towards it, trying to understand it and giving the author the benefit of the doubt if we do not at first succeed. Or we can regard it as a trespass on our time and a threat to our peace of mind and self-esteem, and so do our best to strangle it at birth without giving ourselves the trouble of trying to understand it.

These somewhat gloomy reflections are suggested by reading Professor J. B. S. Haldane's remarks under Notes and Comments in this *Journal*,[1] under the title *A Logical Basis for Genetics ?* in which he does me the honour of discussing my article *What do we mean by 'inborn' ?* which appeared in this *Journal* three years ago.[2]

My article was extremely modest and unpretentious in its aims. It did not offer a basis for anything, let alone genetics. As I pointed out in it, it was almost entirely occupied with embryological considerations, genetics hardly entering into it at all. Moreover, if it had been a basis for something I should not have called it a logical basis because I should not have known what a logical basis was. But unfortunately Professor Haldane's misunderstandings go far deeper than the title. When I first quickly read his remarks I hoped that they would be a valuable contribution to an obscure and somewhat bedevilled topic. But when I read and re-read more carefully, and especially when I compared his quotations with the original article, I was shocked to discover how profound was the misunderstanding and that all his great learning had been directed towards an imaginary object. But when we consider the extent to which the term 'inborn' is used in medical, legal, and sociological literature, the topic seems to be sufficiently important to call for an effort to remove some of Professor Haldane's difficulties.

My article proceeded from the following hypothesis which, for brevity, I shall refer to below as the hypothesis H :

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[1] 1955, **6**, 245–248

[2] 1953, **3**, 319–326

> *What develops from an egg up to a given moment after fertilisation but before death depends on both the kind of egg from which development begins and on the kind of environment in which it occurs.*

Some eminent biologists have declared that they accept this hypothesis ; others have explicitly rejected it. Many biologists, both among those who do and those who do not accept this hypothesis, divide what they call the characters of organisms into (i) inborn or hereditary characters, and (ii) acquired characters. The hereditary characters are those which are said to be ' determined by heredity ' (by which seems to be meant ' dependent upon the kind of egg from which development begins ') and the acquired characters are those which are said to be ' determined by environment ' (by which seems to be meant ' dependent upon the kind of environment in which development takes place '). My article was devoted solely to the problem : How can such a distinction be made by those who subscribe to the hypothesis H ? In my discussion I did not speak of characters but, in an endeavour to follow the extensional method as far as possible, I spoke of classes (called phenotypes) of the organisms which are said to *have* the characters in question. I suggested dividing phenotypes into two groups which I called the environmentally insensitive phenotypes and the environmentally sensitive phenotypes. Roughly speaking a phenotype is environmentally insensitive if, providing you start with the right kind of egg, you will obtain a specimen of the phenotype in question if you give it an environment in which it can develop—one which meets its nutritional and respiratory needs, does not involve immersion in boiling oil or strong sulphuric acid, and so on ; in other words, provided the environment sustains the organism to maturity a member of the required phenotype will appear. In contrast to this a phenotype will be called environmentally sensitive if, given the right kind of egg, a member of the required phenotype will only appear in a rather special kind of environment, so that in another kind of environment which may be equally capable of sustaining the embryo to maturity no member of the phenotype in question is obtained.

I also pointed out that we could have a corresponding division of phenotypes into zygotically insensitive and zygotically sensitive. Combining the two classifications together we get, instead of the two-fold classification, a four-fold one into :

(i) Phenotypes which are both environmentally and zygotically insensitive
(ii) Phenotypes which are environmentally insensitive but zygotically sensitive
(iii) Phenotypes which are environmentally sensitive but zygotically insensitive
(iv) Phenotypes which are both environmentally and zygotically sensitive.

All this seems innocent and harmless enough, but instead of being content with a rough and ready exposition I tried to make my definitions more precise and explicit. My reason for doing this was because on analysis these notions turn out to be rather complicated, and in order to know what they commit us to it is necessary to work out their consequences in conjunction with current biological doctrines. But to work out their consequences when they are complicated we need a calculus, so in order to be able to use the calculus of classes I formulated my definitions with the help of some devices belonging to the theory of classes.

Before I explain these devices I must say a few words about the structure of definitions, because Professor Haldane has misunderstood the structure of my definitions. A definition consists of two parts. One part, called the definiendum, contains the expression to be defined ; the other, the definiens, does not contain the expression to be defined but only those with the help of which it is to be defined. If the expression to be defined is a class designation the two parts are connected by the sign of identity ; if the expression to be defined is a statement-forming functor (see below) then the two parts are connected by the phrase ' if and only if '. In both cases the two parts are mutually substitutable wherever in the development they may occur.

The symbolic devices I used were of two kinds : functors and variables. These devices are familiar enough in mathematical contexts and have been used previously in articles in this *Journal* ; but although functors occur in ordinary language the same can hardly be said for variables. Functors are signs which do not name anything and mean nothing in isolation, like the word ' and ' in English, but which yield a meaningful expression when combined with either names or statements. For example, if I write a statement in front of the word ' and ' and another statement after it I obtain a compound statement called the conjunction of the two statements. In this usage ' and ' is therefore a statement-forming factor. But I can also write ' oranges ' to the left and ' lemons ' to the right of ' and ' in order to obtain a compound name. The word ' and ' can therefore also be used as a name-forming functor. Among the functors which I used belonging to the theory of classes was a horizontal bar written over a class name. This is a name-forming functor because it yields a new class name which names the class of all elements in the universe of discourse which do *not* belong to the class first named.

The second kind of symbolic device which I used, namely variables, is chiefly important in connection with generalisation. Suppose I have at my disposal only names of natural numbers and the sign of addition and identity. Then I can construct such statements as

$$2 + 4 = 4 + 2 \quad \text{and} \quad 3 + 5 = 5 + 3$$

but I cannot formulate the general law (the commutative law for addition) which they exemplify. But suppose I am permitted to use meaningless letters which are not names but can have names substituted for them ; then I can explain that I shall use the letters ‘ x ’ and ‘ y ’ to represent the names of natural numbers ; and I can explain that the prefix ‘ for all x and y ’ is to indicate that the formula which follows it is to be understood as holding for every substitution of names of natural numbers for the letters. The following formula will then express the general law :

$$\text{for all } x \text{ and } y; \qquad x + y = y + x.$$

Without the prefix and other contextual explanation such a formula would obviously be meaningless. This particular prefix is called a universal quantifier.

Variables can also be used in constructing definitions. Suppose we wish to explain to some one what it means to say that one person is uncle of another person. One way to do this would be by calling attention to an example with which the hearer is familiar. We could say that Uncle Bill is uncle of Tom because he is brother of Tom's mother. Being only a particular example this would obviously be misleading and would not fully meet our requirements. We could avoid this by saying that one person is uncle of another person if there is a third person who is a parent of the second person and of whom the first is a brother. Finally we could use variables for which the names of persons can be substituted. We can say that a person x is an uncle of a person y if and only if there is a person z such that x is a brother of z and z is a parent of y. *The use of variables makes the cross references clear*, and when these cross references become complicated it is very useful to have a device for sorting them out clearly.[1]

I can now point out some of Professor Haldane's misunderstandings. He begins by saying that I use the expression

$$\overline{\mathbf{Dlz}}(X, Y, P),$$

but this is incorrect. I use the expression

$$\mathbf{Dlz}(X, Y, P).$$

As I have explained, I write a bar only over a class designation to denote what is called the complement of the class. But ‘ **Dlz** ’ is a statement-forming functor, not a name-forming one. Consequently writing a bar over it in the context in which I use it violates the syntax of set-theory and results in a meaningless expression. What it means in Professor Haldane's article is not explained. He now goes on to state that this expression means ‘ every X which develops in a Y does so into a P ’. But in spite of the fact that two sentences further on he says ‘ But I have quoted Woodger verbatim ’ this is *not* what I say in my article. In the original, immediately after the above formula, I write (in brackets) ‘ (which may be read : Every X which develops in a Y does so into a P) ’. This parenthesis is inserted for the benefit of readers who like to be able to pronounce a formula ; just as we could say that '$x + y$' may read 'x plus y'. But in neither case is this the definiens ! Professor Haldane ignores the definiens entirely, in spite of his claim to be quoting verbatim ! Instead he takes me to task for the statement in brackets and suggests that 'It might have been better to say 'Every member of X which develops in a member of Y develops into a member of P', and this is precisely what the neglected definiens *does* say ! If I had stated this in the brackets there would have been no need for the definiens because the brackets would have contained the definiens. My purpose was to find a short phrase which could be pronounced easily and would not depart too much from ordinary usage. In ordinary talk we should say 'Every cat chases mice', we should not say 'every member of the class of cats chases some member of the class of mice'. Professor Haldane has thus misunderstood my first definition because he has not recognised the definiens, in spite of the fact that it is linked to the definiendum by the words 'if and only if'. He also misunderstands it in another way because he refers to the definiendum as a statement. Now in this definition the definiendum cannot possibly be a statement because it contains three unquantified variables 'X', 'Y' and 'P', just as 'x is uncle of y' is not a statement because it contains two unquantified variables.

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[1] This aspect of the use of variables is well explained by W. V. Quine, *Mathematical Logic*, 1951, p. 70.

The second of my definitions in the chain which leads to the notion of environmentally insensitive deals with the notion of the zygotic range of a phenotype. Here again Professor Haldane misunderstands and misquotes and entirely ignores the definiens. Again he writes a bar over the functor without explanation. He states in quotes 'only from a member of X . . . do you get a P', and then adds his own words : whatever be the environment. Now in the part he has omitted and represented by three dots it is explicitly stated '*only in* a member of Y' ; his addition thus makes complete nonsense of my definition as he has no difficulty in pointing out. But he does not even refer to the proper definiens ; how then can he judge, as he does, whether my definitions 'have reached precision' ? At the beginning of one paragraph Professor Haldane says that he agrees with me that 'such terms as "innate characters" and "acquired character" no longer serve a useful purpose, if they ever did so'. At the beginning of the next he says, 'But I contend that it is better to continue to use admittedly imprecise terms such as "inborn character", or even "species" rather than to replace them by others which can be shown to be equally imprecise'. But if these terms admittedly no longer serve a useful purpose how can we say it is better to go on using them than others which are offered if we do not first understand the latter ?

I get the impression that Professor Haldane objects to the notion of environmental insensitivity because he believes that there are no phenotypes to which it applies. If he had said that we can never be certain that a particular phenotype is environmentally insensitive because we can never test members of its zygotic range in *all* environments I should have heartily agreed with him. But this applies to most if not to all of our notions. We can never be sure that their current use may not have to be modified in the light of future investigations ; and this does not necessarily detract from their utility. The fact that a person can never be certain that a particular man is his or her father does not necessarily render the classification of parents into mothers and fathers useless. If a notion is dismissed without being understood it is not used, and if a notion is not used we cannot say whether it is useful. It seems clear to me that the notions of environmental insensitivity and sensitivity are used in current genetics. All I have tried to do is to analyse them a little. Their use may be illustrated by the following passage from Srb and Owen :[1]

> If it were true that genetic characters in general were highly sensitive to environment, there would be great difficulty in following patterns of gene segregation by observing the distributions of characteristics among particular progenies. Geneticists try to meet this difficulty by conducting their experiments under carefully controlled conditions, in so far as is feasible. Moreover, organisms are fairly well insulated against quite a few of the possible variations in their external environment. But at least equally important to the success of genetic studies is the fact that many genes express themselves quite uniformly and predictably over a wide range of intensities of the agents of environment ordinarily met. The genotype that produces blue eyes in man, for example, seems to be expressed very much the same irrespective of diet or climate.

According to this, and in my terminology, the phenotype consisting of blue-eyed men is zygotically sensitive but environmentally insensitive. But this does not at all mean that this classification may not some day require revision if someone discovers an environment in which a zygote belonging to the zygotic range of blue-eyed men will develop into a brown-eyed man.

Professor Haldane also takes exception to my remarks in a footnote about the suggestion that we should speak of genetic and environmental *differences* instead of characters. Similarly Gates [2] writes :

> I first pointed out many years ago (Gates, 1915) that ' genes ' are essentially *differences*, arising in a chromosome.

Let me explain my attitude a little more fully. If someone should say :

There is a difference between gene *a* and gene *b*

I should understand what was meant because I should suppose it to be an alternative way of saying :

gene *a* differs from gene *b*.

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[1] Srb and Owen, *General Genetics*, London, 1953, p. 80

[2] R. R. Gates, *Human Genetics*, London, 1946, Vol. 1, 6n

But I should find it very hard to understand what was meant if someone should say

gene *a* is a difference.

In the same way I could understand

Tom is father of Dick

and even

There is fatherhood between Tom and Dick

although it would seem rather strange. But I should have considerable difficulty with

Tom is a fatherhood.

Contrary to what Professor Haldane says I should completely agree with anyone who should say : let us work with the notions ' *x* differs genetically from *y* ' and ' *x* differs environmentally from *y* ' because this is the procedure I followed in my *Axiomatic Method in Biology* (1937). Having tried this I am now trying another method. When both have been worked out we shall be in a position to say which is the better one.

I cannot agree with Professor Haldane's remarks about the notion of ' class '. Geneticists frequently use it and Professor Haldane himself uses it. In the 1945 edition of the *Encyclopædia Britannica*, vol. 11, p. 485, he writes :

> A class of organisms whose members cannot be distinguished from one another by observation is called a phenotype.

Of course there are difficulties about the notion of class when it is used in natural science (I have myself discussed some of them in this *Journal*) just as there are difficulties with other very general notions. There are difficulties about the notions of point, straight line, and plane, but these do not prevent Euclidean geometry from being used for the Ordnance Survey. In the same way the calculus of classes can be useful in natural science. But if geneticists use the notion of class why do they not use the calculus of classes ? It can hardly be because they have not heard of it, because it has been available since 1847. It cannot be because it has been tried and found wanting because it has not been tried. And it cannot be because it has been tried and found difficult because it is not difficult.

Let me add that I am by no means wedded permanently to the definitions given in my article on ' inborn '. Since that was written I have devised a better way of dealing with the problem which will be published in due course in an axiom system for the undefined notions involved. For it is only within such a system that definitions of these kinds can profitably be discussed.

J. H. Woodger

The Time of Physics and Psychology

THIS note is in answer to some criticisms by Professor Mundle of my work on the above topic ; I shall come to these later, but I wish first to present a general argument, relevant to his criticisms.

Physical theories which use a five dimensional space-time manifold had a limited popularity during the 1920s. Kaluza started this train of thought with his unitary field theory.[1] The five-dimensional theory of relativity was developed by Klein and others, such as W. Wilson, J. W. Fisher, and H. T. Flint in this country.[2] However, it was never taken very seriously by the majority of writers concerned with mathematical physics. For example, Whitehead wrote disparagingly of those who postulated a five-dimensional manifold ' which has never entered into experience '.[3] Whittaker writing of five-dimensional theories remarks that ' the variations of the fifth variable are not perceptible to us, their non-appearance in ordinary experiments being due to a kind of averaging over it '. De Broglie made similar remarks about the fifth dimension ; and in general physicists and applied mathematicians have come to regard the use of five-dimensional analysis as an artful dodge having no physical significance.

One reason for this attitude towards the physical significance of the fifth dimension has been that all those who have used five-dimensional theory have introduced an expression for the line-element, defining the invariant space-time interval, in which the fifth variable is space-like rather than time-like. I think we should all agree that it is intuitively obvious that ordinary physical space has only three dimensions. But it does not seem to have been generally noticed that, as formally a real space-like co-ordinate is equivalent to an imaginary time-like co-ordinate, the fifth dimension might be treated as an imaginary time [4] in the Minkowski manner.

Prior to the general acceptance of Minkowski's development of Relativity Theory, physicists would have thought the statement that ' the macroscopical world, at any rate, is *four*-dimensional ' [5] (rather than *three*-

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[1] Kaluza, *Berlin Sitzber*, 1921, p. 966

[2] For references see E. T. Whittaker, *History of the theories of the Aether and Electricity*, Vol. II, p. 191

[3] A. N. Whitehead, *Essays in Science and Philosophy*, p. 247

[4] Some theoretical reasons why physical space has only three dimensions have recently been mentioned by Dr G. J. Whitrow in an interesting paper, in this *Journal*, 1955, **6**, 13. Whitrow's argument, which turns on the conditions for approximate stability of the Earth's orbit round the Sun, does not of course invalidate the Minkowski conception of physical space-time as a homogeneous four-dimensional manifold in which the fourth dimension is an imaginary time. Whitrow's argument does not in fact apply to any time dimension either real or imaginary.

[5] P. Bergmann, *Introduction to the Theory of Relativity*, p. 273

dimensional) as absurd as most physicists today think the statement that it is *five*-dimensional. The laws of Newtonian physics were formulated in such a way that the time co-ordinate was set apart from the spatial co-ordinates. Minkowski's development of relativistic transformation theory removed this privileged position of the time; and a displacement became a *four*-vector, the fourth component of which could be interpreted as the relativistic counterpart of the classical Newtonian scalar time. Physicists were eventually driven to accept the four-dimensional view as a consequence of the Lorentz-Einstein-Minkowski transformation theory which was required to surmount certain difficulties in the Newtonian theory. Similarly today we can remove a basic difficulty of four-dimensional physics by recognising that space-time is five-dimensional. Flint has shown how certain quantities, which diverge in the current formulation of relativistic quantum theory, can be treated according to *five*-dimensional theory in such a way that the divergencies of the four-dimensional theory are avoided.[1] But whereas the fourth component of a displacement can easily be interpreted physically in terms of the time component of commonsense experience and of pre-relativity physics, the matter is more difficult in five-dimensional physics. As Flint says,

> One of the difficulties of the present theory has been to interpret the fifth component of any vector which appears in the five-dimensional continuum. . . . It appears that the co-ordinate x^5 is not so easily interpreted as the fourth component x^4 in the theory of relativity.

In my analysis of the specious present phenomenon[2] I have given reasons for rejecting the axiom that the 'macroscopical world at any rate is four-dimensional', by giving a simple interpretation of the fifth component in terms of a directly observable second time dimension; and I have suggested how variations in the fifth dimension of the spatio-temporal frame can be recognised as falling within the field of ordinary sense perception, and therefore as belonging to the macroscopical world. I think it is clear that until physicists are prepared to recognise that a second time-like dimension is actually manifested in every day experience, and so is implicit in their observational data in the way I have suggested, no unitary theory which is based on a five-dimensional space-time manifold is likely to be taken seriously.

However, it may be felt my two-dimensional time-theory of the facts of everyday experience is itself open to serious objections. Some readers may recall that Professor Mundle has made[3] a number of critical comments upon Part I of my paper referred to above. I have no space here to deal with all his criticisms. Some need no answer since they are obviously occasioned by a misunderstanding of my theory, which could have been avoided by reading Part II of my paper (for example, Mundle's surprising assertion that I do *not* hold physical events to be ordered in phase-time: a large portion of Part II of my paper was devoted to a discussion of the phase-time relations holding between physical events).

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[1] H. T. Flint, *Phil. Mag.*, 38, 22

[2] This *Journal*, 1951, 2, Nos. 6 and 7

[3] This *Journal*, 1954, 4, 331-337

But there are two basic points in Mundle's criticism which should be answered. These basic criticisms are that: (i) there is no 'specious present' problem; (ii) no new conclusions can be drawn from my theory which could not be derived from the conventional one-dimensional theory of time; I shall take these points in turn.

(i) The basic problem of the specious present can be put as a question: How can an event, such as a twinge of toothache or a throb of headache, which has a finite duration with manifestly successive phases, be sensibly presented and be *directly* apprehended as a compresent whole, on the orthodox specious present theory (which assumes only a one-dimensional theory of time)? It is no answer to this question to say as Mundle does: 'According to the latter (i.e. the orthodox one-dimensional time theory of the specious present phenomenon) to say that O's awareness of event *a* is simultaneous with O's awareness of event *b* does not entail that *a* and *b* are themselves simultaneous'.[1] For this statement is obviously false in the case of somaesthetic sensations. For example, if O's awareness of event *a* is an awareness of (say) a twinge of toothache; and if that awareness is asserted to be simultaneous with O's awareness of event *b*, which is (say) a throb of headache; then it *follows logically* that event *a must* be simultaneous with event *b*. For it is surely nonsense to suggest that the twinge of toothache felt by O may 'actually have *preceded*' the throb of headache, but O nonetheless '*felt them simultaneously*'. To suppose otherwise is to confuse directly experienced simultaneous events with their hypothetical physical *causes* (which of course need not be simultaneous).

Moreover, to talk glibly of a successively presented series of event-phases as being experienced as a 'gestalt', instead of using Russell's more revealing phrase 'complex of compresence', does nothing to solve the genuine problem of the Specious Present—the problem of how the phases of an event can be both successive and simultaneous in an experient's consciousness: the problem, that is, of 'Experienced Integration'. According to the formulation given by Fessard,[2] 'the very fact that a quick succession of neural activities forms the basis for a single Experienced Integration is . . .

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[1] Mundle accused both Dr Scott-Blair and myself of overlooking this point. Speaking for myself, I had not overlooked it. But I should be very surprised if Scott-Blair had overlooked what is a commonplace of astronomy and physics.

[2] Delafresnaye, *Brain Mechanisms and Consciousness* (Symposium of Council for International Organisations of Medical Sciences), Oxford, 1954, pp. 200-236

an enigma'. He remarks that 'Transient organisations of the conscious present (i.e. the "specious present") do have a "thickness in time" which cannot be neglected. One-tenth of a second, or slightly more, is the common order of magnitude.' Fessard says that these transient organisations are stationary states during which the *passing* of time has no meaning; i.e. although they have a thickness of time, comprising a succession of phases in one sense, yet in another sense these phases are all simultaneous. In describing the neurological problem posed by this 'spread of compresence' or 'thickness in time' of these 'stationary states', Fessard points out that the views of P. Auger may throw some light on the enigma. P. Auger, Professor of Physics at the Sorbonne, has views on the problem of time in current psychology and physics which are remarkably similar to those put forward by myself; though Auger's views were reached independently and were put forward subsequently to mine.[1] Auger too has been struck by the need to distinguish the time associated with stationary states from the time of abrupt transitions between stationary states.

(ii) The essence of Mundle's other basic criticism is contained in the following sentence: 'I conclude then that Dobbs' two-dimensional theory of time explains no psychological data which cannot be explained on a conventional theory.' Specifically Mundle claims that I am unable to explain 'pre-cognition' because I have put forward 'an important axiom' which 'is not included in Broad's analysis of Dobbs' theory. This 'axiom' is apparently the assertion that on my two-dimensional time theory 'there is no reason to think that two separate fluxes of becoming must flow independently', i.e. my rejection of a 'double-now'.

As to the capacity to *explain* precognition, my theory is on exactly the same footing as J. W. Dunne's. Indeed my theory has some resemblance to Dunne's, the chief points of difference being:

(*a*) that I reject his infinitely regressive series of time dimensions and his 'observer at infinity';

(*b*) I postulate only two dimensions of time;

(*c*) there is nothing in Dunne's theory which corresponds to my conception of 'time-uncertainty' (i.e. my idea that the time-spread in a specious present is to be regarded as analogous to a 'dispersion' in the statistical sense).

Dunne rejected, as I do, the conception of a 'double-now'; and also the occurrence of *genuine* precognition. As Broad has remarked in his essay 'Mr Dunne's theory of Time':[2]

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[1] For P. Auger's views see *L'homme microscopique*, Paris, 1952, especially Chapter III

[2] Reprinted in C. D. Broad, *Religion, Philosophy and Psychical Research*, London, 1953, pp. 68-85

> It is extremely important to notice that, on this theory of ' precognition ' no event ever is ' precognised ' in the strict and literal sense. The dreamer who has a veridical precognitive dream is *not* acquainted in his dream with that very same event which later on will happen and fulfil his dream.

This is not the place to go into the details of the way in which a theory of this kind can explain cases of *apparent* precognition. Nor is this necessary as the matter has been very fully and lucidly discussed by Broad. But I must comment on Mundle's statement that

> the new axiom makes Dobbs' model more similar to our simpler model than appears in Broad's appendix for this axiom corresponds to the assumption that a momentary act of sensing is simultaneous with the later boundary of its content. . . .

The word ' simultaneous ' in this context can only mean ' occurring at the same point of time '. But in my theory a *momentary* act of sensing will be simultaneous, in this sense, not only with the *later* boundary of its content but also with the *earlier* boundary, and with all intermediate phases. For, in my theory, all the successive phases of the content of a specious present are compresent together at a single point of time (a moment) in the second time dimension. It follows that two event-phases occupying successive instants t_1 and t_2 in the first time dimension will be simultaneous whenever they belong to the same moment in the second time-dimension. Nothing corresponding to this aspect of the relationship of simultaneity can be found in Mundle's ' simpler model ' : for when time is only one-dimensional, it is logically impossible for two event-phases to be both successive and simultaneous in *one frame of reference*. (Two event-phases can of course be simultaneous in one frame of reference, while being successive in another frame of reference, according to Relativity Theory.)

H. A. C. Dobbs

Defence Department
Kuala Lumpur
Malaya

REVIEWS

SOME USES OF METAMATHEMATICS

Introduction

THERE is no reason to be concerned about the criticism : metamathematics (mathematical logic, the theory of proofs) is useless. After all, if the critic fails to find this interesting subject attractive, it is his loss, not ours. In fact, it is amusing to observe that several mathematicians and philosophers who have not studied this subject in detail, are nevertheless intrigued by it : they do not ignore it, but excuse their ignorance by claiming that it is useless. What is amazing is the reaction of those mathematical logicians who are rattled by this criticism into searching for new results which, though stated in words familiar to the critic, ' cannot ' be got without the help of metamathematics.

The reaction is unreasonable, except on psychological grounds (cravings for affection or influence) which do not concern us here. First, though even an able man may fail to see the latent possibilities of a new idea, experience develops judgment : if one is really *struck* by a new approach, the chances are that results will turn up. Second, a new subject has to be developed a great deal before it can be expected to contribute to old established fields ; remember the time lag between the appearance of the theory of relativity and its most striking application. Third—and this is much more important—even if new results are available (and, of course, they are welcome!), exclusive emphasis on them is bad ; it hides another type of contribution which a new subject can make to a long established field, namely, a better understanding of it by means of concepts which are, initially, foreign to it.

It seems to me that such a situation has arisen in the last ten years with regard to metamathematics. A few fundamental results of the thirties have been so far developed that they can be linked up with older branches of mathematics, particularly algebra. One result of this kind, the so-called completeness theorem of the first-order predicate calculus, has been exploited particularly successfully, to a large extent by the author of this book.[1] Several new results in algebra have been obtained by him and others. Now, it is hard to judge how difficult it would be to obtain these results without the use of metamathematics, though there is some evidence in the literature which I shall mention below. Further, if a mathematician hits on an interesting new result, and proves it by a particular method, this often reflects as much glory on him as on the method. In short, delicate psychological considerations are needed to weigh up these results. What is quite indubitable, however, is the power of the author's investigations for understanding the nature of a very wide class of problems in algebra; merely by looking at the *form* of these problems (more precisely, their hypotheses), one can reduce them to apparently much simpler problems; i.e. if the latter, which usually involve *finite* subsets of those hypotheses, are solved, the solution of the original problems can be inferred by a uniform method: so to speak, the core of the mathematical difficulty has been isolated. The most natural development of this principle, it seems, uses metamathematical methods, and the characterisation of the class of problems concerned certainly needs the concepts of mathematical logic.

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[1] Abraham Robinson, *Théorie métamathématique des idéaux*, Paris, 1955

The methods under review constitute probably the most elaborate application of metamathematics to other branches of mathematics. But some readers may be disappointed, since the main applications concern abstract algebra, a subject which, at any rate in this country, is not very familiar even to those scientists who have a respectable mathematical education. Also, there are other applications of metamathematics. It seems desirable to begin with a pot-pourri of such applications.

Simple Applications

A very simple example is the principle of duality in a propositional logic. Suppose E and F are formulae of propositional logic whose logical constants are $\&$, $\vee$, $\neg$; $E^\star$, $F^\star$ are their *duals* obtained from E and F by the interchange of $\&$ and $\vee$. Then, if $E \longleftrightarrow F$ can be proved (= is a propositional identity), so can $E^\star \longleftrightarrow F^\star$. Note particularly that this does not mean that $(E \longleftrightarrow F) \rightarrow (E^\star \longleftrightarrow F^\star)$ is a propositional identity: if E is $A \& B$, F is B, i.e. $E^\star$ is $A \vee B$, $F^\star$ is B, we get a counter-example. (The duality principle states : if $E \longleftrightarrow F$ holds for *every* distribution of truth values on the atomic formulae of E and F, so does $E^\star \longleftrightarrow F^\star$. $(E \longleftrightarrow F) \rightarrow (E^\star \longleftrightarrow F^\star)$ means: if $E \longleftrightarrow F$ holds for any *one* distribution of truth values, $E^\star \longleftrightarrow F^\star$ holds for *this* distribution too. The latter is evidently stronger than the former.) The principle can be established by considering truth distributions, so to speak the meaning of propositional formulae and not their proofs. But it can *also* be established by first setting up a formal system for the propositional calculus whose axioms are dual in pairs, then showing that the theorems of this system are just the propositional identities, and, finally, converting any given proof of $E \longleftrightarrow F$ into a proof of $E^\star \longleftrightarrow F^\star$. This procedure is metamathematical, since it uses a description of the methods of proof.

A more interesting principle of duality occurs in projective geometry (e.g. of the plane) which uses the geometrical primitives (i) point P, (ii) line l, (iii) P, P' lie on l, (iv) l, l' go through P. The dual of a proposition of projective geometry is obtained by interchanging (i) and (ii), (iii) and (iv). Again, by examining the axioms (of incidence) and proofs it is seen that the dual of any *theorem* is a theorem. Gergonne (1771-1859), who first stated this principle, quarrelled with Poncelet (1778-1867), who objected that it was merely a special case of the method of reciprocation in a conic (whereby poles and polars with respect to a conic are associated). We can now state the merits of the case as follows : Gergonne's version is more general[1] than Poncelet's because it does not presuppose the theory of poles and polars, in particular the theorem that the polars of points on a line are concurrent ; this theory requires Pappus' theorem, and therefore cannot be developed on the basis of the axioms of incidence alone. On the other hand, Poncelet's principle is stronger since it allows us to prove that any proposition (not necessarily a theorem) of every projective geometry implies its dual. (The difference was explained above with reference to duality in propositional logic. The difference is more interesting here since projective geometry is not decidable.) Both the statement and justification of another useful principle due to Poncelet, the method of projection into the circular points at infinity, benefit from a metamathematical approach. It is clear that before the development of metamathematics, the modern justification of Gergonne's principle was out of the question since the notion of 'proof' was not defined syntactically ; though the principle follows fairly clearly from the informal notion of proof to which we shall return below. On the other hand, Poncelet's method of reciprocation could be handled by means of the concepts of his time.

Another example of clarifying an old mathematical dispute by means of metamathematics concerns the distinction between algebraic and analytic proofs of algebraic theorems. Consider first an algebraic proof of

$$x^{p/q} - 1 \leqslant (p/q)(x - 1),$$

where p and q are integers, $0 \leqslant p \leqslant q$, and $x^{1/q} > 0$. Since $a^p - 1 = (a - 1)(1 + a + \dots a^{p-1})$, for $a > 0$, taking $a \geqslant 1$ and $a < 1$ separately, we have $a^p - 1 \leqslant pa^p(a - 1)$, i.e. $p(a^{p+1} - 1) \geqslant (p + 1)(a^p - 1)$, and hence

$$p(a^q - 1) \geqslant q(a^p - 1).$$

The substitution of $x^{1/q}$ for a yields $x^{p/q} - 1 \leqslant (p/q)(x - 1)$. The same result can also be proved from the mean value theorem of the differential calculus as follows : $x^{p/q} - 1 = (p/q)(x - 1)\xi^{-1+(p/q)}$, where ξ lies between 1 and x ; for $x \geqslant 1$, $\xi^{-1+(p/q)} \leqslant 1$ and $x - 1 \geqslant 0$, hence $x^{p/q} - 1 \leqslant (p/q)(x - 1)$; for $x \leqslant 1$, $\xi^{-1+(p/q)} \geqslant 1$ and $x - 1 \leqslant 0$, and hence, again, $x^{p/q} - 1 \leqslant (p/q)(x - 1)$. Mathematicians who prefer algebraic proofs sometimes claim that the use of the differential calculus restricts the *generality* of the proofs. Now, the position is this. From an analysis of Sturm's theorem Tarski has obtained a complete system in which every real algebraic proposition can be *decided*; thus, if such a proposition can be proved in the differential calculus, i.e. is true in the field of real numbers, it can be proved in Tarski's system, and is therefore true in *every* real closed field. So, the mere fact that there exists an algebraic proof of such a proposition gives us no more information than a proof in analysis: in this sense the use of the latter does not restrict the generality. On the other hand, as a general rule, the algebraic proofs of informal mathematics use only very few of Tarski's axioms (closure conditions), and their generality compared with analytic proofs depends on this last fact.

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[1] I do not know a proposition (in the symbolism of projective geometry) from which its dual cannot be proved by means of the axioms of incidence. Here, as in the remaining discussion of the duality principle in geometry, it is assumed that all variables are bound ; in contrast to the propositional case.

The mere classification of propositions by their quantifiers leads to sound judgments on the constructive character of theorems in analysis. If $A(n)$ can be decided for each numeral n and the proof of $(Ex)A(x)$ (there is an x with the property A) is correct, there must be a numeral n with the property $A(n)$. One can rely on it that even if the proof of $(Ex)A(x)$ contains undecided disjunctions (the usual source of non-constructive theorems), examination of the proof will yield a reasonable bound for n. (Actually, this principle with a precise meaning of 'reasonable' has been established only for proofs which do not use a strong set theory; but since few proofs of arithmetical theorems use the axiom of choice or impredicative sets, this principle is not likely to let one down.) On the other hand, even if $(Ex)(y)A(x, y)$ has been proved, or if there is a (numeral) n such that for each m we have $A\ (n, m)$, we cannot expect to prove $(y)A(n, y)$ by means of the methods which provided a proof of $(Ex)(y)A(x, y)$. Now, suppose we have a sequence of rationals $a_1, a_2, a_3, \ldots$ where each a_n can be calculated systematically, and a proof that $\lim a_n = 1$. We cannot expect to get a bound N such that for all $m \geqslant N$, e.g., $|\ 1 - a_m\ | < 1$: for, this means $(m)(m \geqslant N \rightarrow |\ 1 - a_m\ | < 1)$ while we have only proved $(En)(m)(m \geqslant n \rightarrow |\ 1 - a_m\ | < 1)$. On the other hand, if $a_1 \leqslant a_2 \leqslant a_3 \leqslant \ldots \leqslant 1$, then, for any positive ϵ, we expect to find such an N: for, it is sufficient to find an N satisfying $|\ 1 - a_N\ | < \epsilon$, since $a_N \leqslant a_{N+1} \leqslant \ldots\ 1$; we have a proof of $(En)(|\ 1 - a_n\ | < \epsilon)$, since $\lim a_n = 1$ has been proved, and we can expect to get a bound for N. More generally: if we have proved that infinitely many integers have the property A then, for any n, we expect to get a bound for an integer $> n$ with the property A: for, we have $(Em)[m > n\ \&\ A(m)]$. On the other hand, if we have proved that all sufficiently large integers have this property, we cannot expect to get information about how large these integers must be: for, we have only proved $(En)(m)[m \geqslant n \rightarrow A(m)]$. In technical language, 'we cannot expect' means: there is no recursive method.

A very different type of application derives (deviously) from a famous result in metamathematics which, very roughly, may be stated as follows:

any theorem which is true when the variables range over *all* real numbers, is also true when they range over certain ' thin ' subsets of the continuum. For instance, there is a well-known theorem of analysis : if a function is continuous at each x of $[a, b]$, i.e. each x satisfying $a \leqslant x \leqslant b$, it is uniformly continuous in $[a, b]$. For, if f is not uniformly continuous in $[a\ b]$, but continuous at each point of $[a, b]$, it is not uniformly continuous over the rationals of $[a, b]$; in this case a certain sequence of rationals (depending on the values of the function at the *rationals*) can be defined which converges to a point ξ of $[a, b]$ where the function has a discontinuity. But the argument shows more : let S be *any* set of points of $[a, b]$ which contains the rationals and ξ ; then, if the function is continuous at each point of S it is uniformly continuous over S. Of course, this generalisation is *very* easy (as is the generalisation that the theorem is true in any compact space!). But the point is that a new *type* of generalisation is introduced : we do not assume compactness in the sense that *every* infinite subset of the space has a limit point, but restrict ourselves to certain subsets specified in advance (here, the sequence with the limit ξ). Much of Lorenzen's work on the foundations of analysis leads to generalisations of the type described in this paragraph (though he himself would emphasise a different aspect of his work).

A consequence of Gödel's work on the consistency of the axiom of choice and the continuum hypothesis is this : if an arithmetical theorem can be proved in standard set theory from these axioms it can also be proved without them : one ' relativises ' the proof to so-called constructible sets and classes, and observes that an arithmetical theorem is its own relativised form since the integers are absolute. In particular, this applies to recent work on the order of homotopy groups where the axiom of choice is applied to certain vector spaces of the power of the continuum ; for, by means of simplicial mappings, the order of a homotopy group can be expressed in an arithmetical proposition. The constructible elements of the vector spaces considered are non-enumerable: whether they can be replaced by enumerable subsets along the lines of the preceding paragraph is an open question.

The reader will have noticed that the duality principle has the general look of a mathematical theorem, while the style of the remarks on arithmetic and analysis jars with that of orthodox proofs in these subjects ; also, the remarks are very simple. This situation is quite natural. Projective geometry has been developed as an axiomatic (formal) system, dominated by metamathematical ideas ; or, at least, by a conscious interest in methods of proof. Metamathematical investigations which use a formalisation of these proofs, are in the spirit of the subject. On the other hand, formal systems provide only very *rough* approximations to arithmetic and analysis ; moreover, most of the *detailed* information on such ' approximations ' concerns the particular systems used more than the informal branch of mathematics under discussion. From the study of the systems emerge *guiding* rules for understanding informal proofs, perhaps a little like working out ' ideal ' cases in fluid dynamics : one accompanies the calculation by a suitably colourful commentary, and is then ready to try one's hand at some ' messy ' problem like understanding the behaviour of sea waves near a coast line or the noise made by propellers. The calculation may involve severe mathematical difficulties ; but here too only a small fraction of these has any very *direct* bearing on the application. The organism of informal proofs in arithmetic and analysis is messy too : these subjects have not been kept pure and sterile for 2000 years by a germicidal system of axioms, like Euclidean geometry.

From now on we shall be mainly concerned with axiomatic systems.

Models

Perhaps the most famous application of metamathematics is (Hilbert's version of) the proof of the independence of the parallel axiom : it cannot be proved from the remaining axioms listed by Euclid. This was shown by re-interpreting the words ' straight line ' and ' congruence ' (which appear in the axioms) so as to satisfy the remaining axioms, but not the parallel axiom. Now, though this re-interpretation was given long before the subject of metamathematics was thought of, yet, at first, one feels that before Hilbert the result rested on a very shaky foundation : we claim that the axiom *cannot* be proved from the remaining axioms without having said a single word about what proofs are to be considered! (Mathematicians are more upset about this than the plain man who, every day, has occasion to say that he cannot do this or that, but never produces a list of all-the-unsuccessful-methods which he might have tried.) I think even in those early days the situation was not hopeless : in fact Bolzano, as Scholz has stressed, had a good answer, namely :

Whatever other conditions may be imposed on, or fulfilled by, *proofs*, a correct proof must be such that if a given model (example) satisfies the premiss it also satisfies the conclusion.

Observe in passing that Bolzano's remark covers Gergonne's duality principle : since the axioms are satisfied when one reads the word ' point ' as line, ' line ' as point, etc., so are the conclusions.

We can illustrate both the use and the limitation of Bolzano's remark by means of simple examples. First, it covers one of the most common ways of showing that a proposed inference is wrong, e.g. if $A \to B$ then $\neg A \to \neg B$: put $a = b$ for A, $ac = bc$ for B, when $A \to B$ is satisfied ; now take $c = 0$ and a different from b, when $\neg A \to \neg B$ is false. Next, the remark loses its importance—in fact the whole question of logical validity becomes less interesting—if one allows unintended and trivial exceptions; in the language of logic, if one is prepared to alter one's premisses as one goes along. But does this happen ? Or is such a lapse itself a trivial exception in the development of mathematics ? It seems that it happens frequently in informal mathematics ; e.g. if a counter-example is artificial one slightly rewords the conditions [1] and forgets about the defective inference : this would ordinarily happen if a physicist were given Dirichlet's function ($= 1$ for rationals, $= 0$ for irrationals) as a counter-example. In this connection, it seems that the break between Euclidean and non-Euclidean geometry became final in the informal development of mathematics, not so much because the parallel axiom is independent of the remaining axioms, but because there are non-Euclidean geometries which are easy to grasp (worth pursuing). [1] Only when one takes the axioms as immutable, e.g. if no different sets of axioms are given the same name, does Bolzano's remark apply without further reservation. (It goes without saying that Bolzano's remark is irrelevant to distinctions between different sorts of proofs, e.g.

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[1] Modification of axioms and definitions—which is, of course, not done by following some other set of axioms—is a difficult and interesting part of mathematics. For instance, a surface was defined to have an area if the limit of the surface areas of inscribed polyhedra with triangular faces exists as the length of their edges tends to 0 ; this limit is then the area. Schwarz showed that a circular cylinder did not have an area : the consequence of this discovery was not that *two* theories of area were pursued (like Euclidean and non-Euclidean geometries), one in which a cylinder did not have an area, and one where it had its ' naive ' value. A more highbrow example in the same field is Besicovitch's assault on Lebesgue's definition : there are surfaces which have an area (on this definition) which is smaller than the area of one of its projections. It is unfortunate that the Gödel incompleteness theorems are usually compared with the independence of the parallel axiom, and not with such mathematical phenomena as gradual improvements in the definition of surface area. The former comparison is doubly unsuitable : (i) there are non-Euclidean geometries which are very simple, but given a sufficient set of axioms any definition of so-called non-standard integers needs predicates which are not recursively enumerable, i.e. complicated ; note that the current defence of Lebesgue's definition of *area* against Besicovitch is this : the latter's surface is considered too ' complicated '—it does not satisfy certain conditions which, be it noted, were imposed explicitly only after his construction was published ; (ii) the choice between a Gödel undecidable proposition $(x)A(x)$ and its negation $(Ex) \neg A(x)$ is made on a very simple principle (letting quantifiers range over the numerals) while no simple principle suggests itself for the choice between Euclidean and non-Euclidean systems. This last point seems to be an interesting subject for speculation : is it that the ' natural ' growth of geometric concepts was inhibited by the years of axiomatic treatment of geometry? It is certainly remarkable that the only choice considered depends on delicate observations of light rays : the arguments for and against a definition of area use formal criteria such as additivity or the requirement that the area of a surface must not be less than the area of one of its projections ; experiments on the measurement of area (e.g. weighing the paint needed to cover a surface) are not brought in.

constructive, elementary, direct: we discussed limitations only in those contexts where it appears at first that the remark might be decisive.)

We have spent some time on models and Bolzano's remarks because they are central to the book review. Robinson himself explicitly states that they do not cover 'all' mathematics: rightly, since in fact they cover hardly any mathematics which the non-specialist knows. But some of the more philosophical readers may have come across facile claims for semantics (models) and be prejudiced against it, because these claims conflict patently with what the reader knows. The purpose of this section was to indicate that models are perfectly natural in connection with axiomatic mathematics, and there one may expect them to be useful; after seeing their limitations one has more confidence in positive claims.

The Predicate Logic of First Order

We shall now consider axioms which are formulated in a first-order symbolism, i.e. we have quantification over individuals, but not over predicates (classes) or functions. A typical example is an axiom system which expresses that a relation O is a total *ordering* relation:

$$(x)(y)[O(x, y) \longleftrightarrow . \, x \neq y \,\&\, \neg O(y, x)], \tag{1}$$

(x precedes y if, and only if, $x \neq y$ and y does not precede x; in particular, x does not precede itself, and if $x \neq y$ either x precedes y or y precedes x).

$$(x)(y)(z)[O(x, y) \,\&\, O(y, z) \, . \rightarrow O(x, z)] \tag{2}$$

(Ordering is transitive).

If (1) is replaced by $(x)(y)[x = y \vee O(x, y) \, . \rightarrow \neg O(y, x)]$ (3)

the order relation need not be defined for all pairs of elements: (2) and (3) state that O is a *partial* ordering.

A series of researches beginning with Frege's construction of a predicate *calculus* (rules of logic), led to Gödel's completeness theorem which established a very close connection between Bolzano's condition *on* correct proofs and proofs generated by Frege's rules.

If $A \rightarrow B$ cannot be proved according to Frege's rules there is an (arithmetic) model for A which does not satisfy B. (Also, Frege's proofs satisfy Bolzano's condition.)

The variant, when A does not occur, is this: if B cannot be proved according to Frege's rules, then there is a model [1] which does not satisfy B.

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[1] For the meaning of 'model': a first-order formula is made up of logical constants (for all), (there is), (and), (or), (not), $=$; and non-logical constants, namely symbols for individuals, predicates, relations, functions of one or more variables. An arithmetic model of such a formula is obtained by replacing the non-logical constants by arithmetic expressions of the same category (i.e. numerals replace the symbols for individuals, arithmetic predicates the predicates, etc.) in such a way that the resulting formula holds in arithmetic.

In view of our discussion on models we must compare this general result with the case of non-Euclidean geometry. First, the models needed are not always as simple as those of ordinary non-Euclidean geometries (in technical language: there are propositions B which cannot be proved in the predicate calculus, but no model of $\neg B$ is recursively enumerable). Second, the logical constants (of A and B) must be interpreted in a way which is closely bound up with the rules of Frege's calculus (the so-called ' classical ' meaning of quantifiers) ; for instance, Gödel's models would not generally ' satisfy ' the proposition $\neg B$ on the meaning which intuitionists give to this word. But Frege's rules are in the spirit of much of informal mathematics, and there too statements are often interpreted ' classically '.

The Fundamental Principle

A very natural question arises at this point. Suppose we ignore the fact that if the axioms are to be written out there can only be a finite set of them. (We have many familiar examples, e.g. if we work with numerical arithmetic the free variable formula $2^{n+1} = 2 . 2^n$ is a schema that could only be ' expressed ' by an infinite set $2^1 = 2 . 2^0$, $2^2 = 2 . 2^1$, . . . , a set which could not be written out ; yet it is a perfectly usable schema for numerical calculations.) Let us then leave open the question how a schema for such axioms is to be formulated : it is understood that the results we get will be more or less informative according to how sharply the schema is phrased. We ask, for a given B and a given set A consisting of first-order formulae (but otherwise arbitrary) : does B follow from A ? According to Bolzano, we have to decide whether every model which simultaneously satisfies all formulae of A satisfies B. On Frege's characterisation of proofs it is not very clear how this question is to be interpreted, except this : the answer would certainly be positive if B can be proved from a suitable *finite* subset of A. (It is clear that if a numerical formula is proved by substitution in free variable formulae, only a finite number of substitutions are made—on the syntactic meaning of ' proof '—and therefore the formula is proved from a finite number of instances of the free variable schema.)

I suppose on first sight one would think that the gap between Bolzano's condition and the tentative syntactic interpretation was far too wide to be bridged. But this is just what has been done ! They are equivalent, provided some order can be put into A, i.e. if A is complicated, one assumes the axiom of choice. Another way of putting the equivalence is this : if $\neg B$ is consistent with every (B does not follow from any) finite subset of A then $\neg B$ is consistent with A.

We call this fact the *fundamental principle*. It is the basis of the type of application of metamathematics to algebra which is considered below. Strictly speaking, the only metamathematics which is needed to establish the principle consists of the symbolism of the first order predicate logic employed in characterising the formulae of A and B. (Examples below will show that the restriction to a first-order symbolism is essential.) For, we can proceed as follows: suppose for each finite subset A_1 of A there are models which satisfy both A_1 and B; then by a topological argument reminiscent of the proof of the Heine-Borel theorem in analysis, we can piece together these models to get a model which satisfies B and A itself. But I think it is fair to call the result metamathematical in the stronger sense which implies that a description of methods of proof (Frege's calculus) is used: for from this point of view the principle is so eminently plausible!

Examples

The examples below are not representative of the book. They are very simple, the type of thing that used to be considered years ago (cf. the author's earlier book which was reviewed in this *Journal* (1953)). The main aim of the present book is to *widen* the applicability of metamathematics: it shows how certain concepts of algebra (ideals, prime ideals, algebraic varieties, etc.) can be ' described ' by means of suitable classes of propositions. These developments do not really come within the province of this *Journal*, but the general ideas do; they can be explained by means of simple examples, and this is why we concentrate on them.

(*a*) *Models and Extensions.* Every partial ordering can be extended to a total ordering.

At first this seems to have nothing to do with the fundamental principle: ' every ordering ' sounds like quantification over relations, which was expressly excluded, the word ' extended ' is not mentioned in the principle, and the words ' consistent ' or ' model ', which occur there, are not mentioned in the present theorem. Now, the argument runs as follows:

Suppose a partial ordering is defined over a set S of elements α (or: named ' α ', if one wants to be fussy). We can ' describe ' the partial ordering by means of its ' complete diagram ', i.e. the following class K of propositions: $O(\alpha, \beta)$ belongs to K if, and only if, α precedes β in the given partial ordering. (To fix ideas we suppose that every element of S occurs as an argument in at least one proposition of K.) Given any *finite* set of elements of S, it is clear that they can be totally ordered consistent with the given partial ordering, i.e. the two orderings coincide for all pairs of elements for which both of them are defined. In other words, for any *finite* subset of K, there is a model of this set of formulae (for the predicate symbol O) which satisfies (1) and (2) above. (In this particular case there is a model whose only elements are the symbols α, β, . . . occurring in the subset; to exemplify the general method we do not use this fact.) By the fundamental principle there is a model which satisfies (1) and (2) and *all* the formulae of K. The model will be defined over a set which includes S; further, wherever a pair α, β is ordered by the given partial ordering this order is preserved in the model (since then $O(\alpha, \beta)$ or $O(\beta, \alpha)$ is in K).

It is quite trivial that an empty K is also a partial ordering: it follows from the fundamental principle that every set can be totally ordered.

Note that the model defined might include elements other than those of S itself. We could ignore those because the axioms (1) and (2) contain only universal quantifiers. If axioms contain existential quantifiers, in general, 'new' elements might be associated in the model with the elements of a given structure, and we should have an *embedding* theorem.

A very similar argument shows that a group can be totally ordered, if every finitely generated subgroup can be totally ordered (here, 'total ordering' satisfies a further condition: if $O(\alpha, \beta)$ and $O(\alpha', \beta')$, then $O(\alpha\alpha', \beta\beta')$). This case is interesting because one can compare a proof without-much-metamathematics by E. H. Neumann, *Proc. Lond. Math. Soc.* 4, 1954, 138–153, and one by Robinson, *Journal Lond. Math.* Soc., 1955, **30**, 249–252.

(*b*) *Models and Colouring*—a picturesque example. (This case also provides a comparison. De Brujn and Erdös, *Konig. Akad. Wetensch.*, Amsterdam, 1951, **54**, 371–373, and this book p. 84.)

To fix ideas consider regions in the plane. Two regions are said to *adjoin* if their frontiers have a point (or a linear stretch) in common. The reader will see that the argument has nothing to do with geometry, and the exact meaning of 'adjoin' is quite unimportant.

Suppose k is a fixed integer. If every finite set of these regions can be coloured by means of k colours so that adjoining regions have distinct colours, then the original set of regions can be so coloured.

Let $C_i(\alpha)$ mean that the region (named) α gets colour i, $1 \leqslant i \leqslant k$, $A(\alpha, \beta)$ that α and β are adjoining regions. The structure of the given regions is described by (the complete diagram consisting of) the class A where $A(\alpha, \beta)$ is in A if α adjoins β, $\neg A(\alpha, \beta)$ is in A if α does not adjoin β.

We can now write down a set of formulae expressing the conditions: Adjoining regions get distinct colours, namely

$$(x)(y)[A(x, y) \rightarrow . \neg C_i(x) \vee \neg C_i(y)], \quad 1 \leqslant i \leqslant k. \tag{4}$$

Every region has a colour, namely,

$$(x)[C_1(x) \vee C_2(x) \vee \ldots \vee C_k(x)]. \tag{5}$$

No region has more than one colour,

$$(x)[\neg C_i(x) \vee \neg C_j(x)], \quad 1 \leqslant i < j \leqslant k. \tag{6}$$

The hypotheses of the theorem state that every finite subset of A is consistent with (4), (5), (6). By the fundamental principle the whole of A is consistent with (4)-(6), which is a paraphrase of the conclusion.

If the theorems (*a*) and (*b*) are considered in isolation, the metamathematical proof of (*a*) is better than the other, in (*b*) there is little to choose between the two ; but if the whole body of related theorems is considered, it is fair to say that the proofs from the fundamental principle give one a much better understanding, particularly as the principle is both *easy* and *useful* ; this last fact explains why so many people have formulated it independently of each other, or, as in (*b*), come near it.

The examples above show incidentally that there is a good deal of scope for ingenuity in ' expressing ' a problem from algebra by means of the notions used in formulating the principle. In particular, though quantification over predicates is not permitted, the *consistency* of a set of axioms containing the predicate symbol F expresses that there *exists* a class (predicate) F satisfying these axioms.

As a quite different application of the fundamental principle, note that every first-order axiom system for arithmetic has also a non-standard (unintended) model. Suppose such a system, containing the constant c and the function symbol S, has a model over the natural numbers where c is replaced by 0, and S by the successor function. Now add to this system the axioms

$$Dc = c, \quad (x)[D(Sx) = x], \quad D^n\xi \neq c, \quad n = 1, 2, 3, \ldots,$$

where D^{n+1} means $D[D^n\xi]$, and D and ξ do not occur among the original axioms. Then every finite subset of the new axioms is (trivially) consistent with the original system, and, by the fundamental principle, so is the whole lot. But the new system has no model over the natural numbers in which c is replaced by 0 and S by the successor function since ξ differs from all the terms obtained by repeated application of S to c. Any model of the new system is a non-standard model of the original one.

Limitations

One cannot express by means of the first-order symbolism the condition : finite and only finite sets have the property P. For, if P is possible in every finite set, P is consistent with every *finite* subset of the class $x_i \neq x_j$, $1 \leqslant i < j$ ($i. j$ integral), and by the fundamental principle consistent with all of them, i.e. possible in an infinite set. (Of course, for any given finite n, it is easy to state that P is possible in a set with n elements, and for no other cardinal).

The example above shows that it is possible to express the condition : P is possible in an infinite set (by means of the infinite set of axioms $x_i \neq x_j$). An alternative way of expressing it follows from Dedekind's definition of infinity : if F does not occur in P, add to P the formula

$$(x)(Ey)(z)[F(x, z) \longleftrightarrow y = z] \mathbin{\&} (x)(y)(z)[F(y, x) \mathbin{\&} F(z, x) \,.\!\rightarrow y = z] \mathbin{\&} (Ex)(y) \neg F(y, x) \qquad (7)$$

P is consistent with (7) if, and only if, P can be satisfied in an infinite set, since (7) itself can be satisfied only in an infinite, and in every infinite, set.

One could express ' finiteness ' by means of quantification over a single relation-variable, namely $(F) \neg (7)$:[1] this shows that the fundamental principle breaks down if the axioms consist not only of first-order formulae, but include such second-order formulae. Another way of expressing ' finiteness ' would use infinitely long disjunctions :

$$(Ex_1)(Ex_2)(x_1 \neq x_2) \vee (Ex_1)(Ex_2)(Ex_3)$$
$$(x_1 \neq x_2 \mathbin{\&} x_1 \neq x_3 \mathbin{\&} x_2 \neq x_3) \vee \ldots \vee \ldots$$

(Another abstraction : we have already ' infinitely long ' conjunctions, namely an infinite set of axioms) ; in this case the principle would break down too.

Two Minor Points

Robinson states that the methods used in the book are non-constructive ; but it is not as bad as all that. Of course if the set A (in the statement of the fundamental principle) is excessively ill-defined, the model he finishes with is ill-defined too ; just as $(A \mathbin{\&} B) \rightarrow (A \vee B)$ carries any ambiguity that may infect ill-defined propositions A and B. For reasonably well-defined classes A, a finitist version of the completeness theorem can be given by means of the ideas of Herbrand's aptly named *théorème fonadmental.* (Of course, the algebraic terminology has to be tightened up too.) Herbrand's treatment is described very clearly in vol. 2 of Hilbert-Bernays' *Grundlagen der Mathematik.* The existence of such a treatment is the best reason for keeping to a non-constructive terminology (such as is employed in the present book) ; it means that we know how to ' read ' the non-constructive terminology from a constructive point of view. It is reminiscent of the ϵ, δ definitions in analysis : once they are clearly understood one uses ' near ', ' small ', etc., quite freely and with confidence.

The second point is a question. Examination of Gödel's own version of the completeness theorem of the predicate calculus shows that the arithmetic models can be defined by very restricted means (in technical language : they belong to P_2Q_2). In metamathematics itself this is of some interest. Is this additional information about the models of algebraic interest ?

To repeat : The main contribution of Robinson's book is mathematical, and therefore outside the field of interest of this *Journal* ; however, the book raises the question of the application of metamathematics. The main purpose of this review was to discuss some aspects of this question.

G. Kreisel

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[1] The function-quantifier ' (F) ' ranges over *all* functions ; or, more precisely, over a class of functions which satisfies suitable closure conditions. It is not surprising that *finite* and *infinite* can be distinguished by means of function-quantifiers, since the definition of a function may presuppose an infinite range.

The Sources of Eddington's Philosophy. By Herbert Dingle.
Cambridge University Press, 1954. Pp. 64. 3s. 6d.

Professor Eddington's views have been discussed many times, with various degrees of comprehension. Professor Dingle's Eddington Memorial Lecture describes them very well, and reinforces his interpretation with many quotations, going back to the Physical Society Report on relativity in 1916. Dingle emphasises that Eddington never claimed to derive anything about the constitution of the world by ' pure reason ' ; his claim was that those regularities that we call the laws of physics are consequences, not of properties of the actual world, but of our method of observation and interpretation, and thus of conventions. Dingle accepts this statement. The physical world, to Eddington, was different from the external world, but could be held to symbolise it. Dingle rejects this, on the ground that the external world (which he mysteriously calls Victorian) never appears in physics, and should be dispensed with altogether. He traces many confusing arguments in Eddington's works to his determination to retain the external world in spite of its apparent irrelevance to physics. Eddington is often called an idealist ; Dingle's objection is that he is not idealist enough.

I do not myself believe that anybody could work an idealism such as Dingle recommends ; I am sure that nobody does so now. On nearly every page of the pamphlet there is a mention of something that we should ordinarily call an ' object '. Possibly an idealist language could be constructed that would avoid this, but it does not exist yet. There is also a systematic confusion in the use of ' we '. If it is to be understood as plural, a person using it assumes the existence of other people, and they are part of his external world. If Dingle carried out his rules consistently he would have to dispense with them.

One of his arguments is that lengths change with relative motion and therefore are not intrinsic properties of bodies. But by comparing bodies in relative rest we can give a meaning to ' A is longer than B ' ; and abstraction of a property from a method of comparison is a normal logical procedure. Lengths of bodies in relative motion are inferred, not directly measured. Relativity introduces no new philosophical problem here ; it is the old one of a view seen through coloured glass. The matter is merely more complicated than people had thought.

Dingle points out that Eddington had stated the essential points of operationalism and logical positivism in 1923 ; but surely all come from Mach. He says (and here I have agreed with him at considerable length) that Eddington's so-called epistemology has included a great deal of observational information before the rules for analysing the observations are laid down at all. I think that there is no hope whatever of clearing up the obscurities in modern physics until a much more fundamental analysis is carried out. There is probably a great deal of good physics in Eddington's *Fundamental Theory*, but at present this is so overlain by bad epistemology that it is very hard to see what it is.

HAROLD JEFFREYS

Readings in the Philosophy of Science.
By HERBERT FEIGL and MAY BRODBECK
Appleton-Century-Crofts, New York, 1953. Pp. ix + 811. $6.00

THIS is a most welcome volume. It will be of considerable value to students —it was even designed as a sort of textbook—for it covers the ground of the subject very well. It also constitutes a convenient source-book of many important contributions that appeared originally in diverse places.

The Readings consist of papers from journals and also of chapters from well known books. Though the aim is to present current approaches to the subject, some of the writings are by authors, such as Mach, who died some time ago. The selections were written almost entirely by contributors from America or the Continent of Europe. It is satisfactory to have certain works by, for example, Einstein, Bridgman, Mach, Poincaré, Duhem, and Schlick, made accessible. One could hardly quarrel about the justice of including any of the works reproduced ; the only ground for criticism would be of omission rather than commission.

The arrangement is natural enough. There are sections, each of several papers, on the philosophy of particular branches of study—mathematics, physics, biology, and the social sciences. And there are three general sections : one deals with special topics, namely, causality, determinism, and probability ; another with the nature of scientific method ; and the third with scientific explanation and theory construction.

It would be impossible to discuss theories from fifty-two different papers. We may note, however, that the approach is modern. A nineteenth century writer of a standard textbook would be surprised at the changes of outlook and topic. Thus there is not a single paper—though the theme does receive mention—attempting to justify induction ; which is as it should be nearly twenty years after the publication of the *Logik der Forschung*. Readers of this *Journal* should at least know about this volume and would do well to possess it.

J. O. WISDOM

Analytische Erkenntnistheorie. By Arthur Pap.
Springer-Verlag, Wien, 1955. Pp. vi + 242. 41s.

The sub-title of this book is ' A critical survey of recent developments in the United States and England '. It is based on lectures given by Professor Pap when he was Fulbright visiting professor at the University of Vienna during 1953-4. In his preface, Pap deplores the fact that Vienna, which was the home of the logical empiricist movement, is now given over, along with most of the rest of Europe, to types of philosophising which he has elsewhere very aptly described as ' edifying obfuscation '. His Fulbright lectures thus represent a crusade to rescue the holy places of empirical philosophy from the hands of the infidel. I hope that his crusade will have the success that the merits of his book deserve.

Pap is, of course, no slavish adherent of the doctrines of the Vienna Circle, or of any other philosophical orthodoxy. It is clear from his other writings that he is one of the most independent and eclectic of the writers who are vaguely called ' philosophical analysts '. His treatment is designed to demonstrate a philosophical method and not to advocate any theories, even his own. The main subjects discussed are the empirical criterion of meaning, perception, truth, probability, causality, explanation, and the nature of logical necessity. He omits some topics which are usually counted part of ' the theory of knowledge ' (memory and conceptual thinking, for instance) and includes causality which would perhaps not usually be included. But his subject matter is, on the whole, a very fair selection of the questions which have been fashionable in England, Scandinavia, and the United States during the past twenty-five years. The most important views are clearly explained and very incisively criticised. Each section has a short bibliography to guide the reader to some leading essays in the field under discussion. Although Pap's aim is to show a philosophical method in action and not to advance his own views, the book contains a good many interesting and original suggestions, modestly disguised as criticisms of the views he expounds.

The only serious criticism I should like to make is that Pap seems to me to over-emphasise the importance of formal logic as a philosophical tool. This leads him, to take one example, to discuss the question of truth in terms of Tarski's semantic theory. Many contemporary philosophers would regard this theory as having no bearing at all on the important problems connected with the notion of truth. Indeed, most philosophers in this country at the present time tend to think of formal logic as a branch of mathematics and of little consequence as an instrument for philosophical investigation, except perhaps in the philosophy of mathematics. (After all, what problems have been given an *agreed* solution by formal methods ?)

But, it is perhaps as well to be reminded that the informal linguistic or conceptual analysis now fashionable in England looks less impressive from Yale or Vienna than it does to us here. However, this is an excellent book. It shows, besides the academic virtues of fairness, clarity, and precision, an enthusiasm for philosophy and a faith in it that is now too rare among professional philosophers.

D. J. O'Connor

The Psychology of Invention in the Mathematical Field. By Jacques Hadamard. Dover Publications, New York, 1954. Pp. xiii+145. $1·25

This scholarly and modest little book is founded on lectures given in New York in 1943 by the author, a distinguished pure mathematician. His treatment is based on the one hand on the examination, in the light of cautious introspection, of what has been said on the subject by a few other eminent mathematicians (notably Henri Poincaré) who have also published the conclusions of introspection into the mental processes by which they discovered important mathematical theorems, and on the other hand on what has been said by pre-Freudian psychologists (much of which, however, the author rejects). It is characteristic of Professor Hadamard's objectivity that he discusses his own failures as well as his successes and characteristic of his insight that he realises the importance of Freud's work, though he is not familiar with it. The result is a most readable book, not conclusive, but full of interesting and thought-provoking suggestions.

M Hadamard lays stress from the outset on the importance of unconscious processes in arriving at the consciousness of new mathematical truths, which (as he emphasises, following Poincaré) are not in fact reached at the end of a chain of conscious reasoning. 'That those sudden enlightenments . . . cannot be produced by chance alone is evident ; there can be no doubt of the necessary intervention of some previous mental process unknown to the inventor, in other terms, of an unconscious one' ; and his second and third chapters are entitled 'Discussions on Unconsciousness' and 'The Unconscious and Discovery'. The distinction is drawn between 'those subconscious states which are . . . accessible to introspection, which, at least in general, is not possible for more remote processes', and those which are not so accessible ; and the author is aware that 'there must even be, in the unconscious, several successive layers, the most superficial one being the one which is available to introspection'. (A footnote here states that the author has been informed that this is not inconsistent with Freud's thought, correctly interpreted.)

He also draws attention to the emotional element in the causation of inventive thought and points out that 'the illumination process is not of the same nature as the previous conscious work '. The discussion of unconsciousness, at the pre-Freudian level, is extraordinarily penetrating, as witness the following quotations :

' Some authors had a kind of fear of the unconscious and were unwilling to admit its very existence '—' The conscious and the unconscious co-operate '—' Can we find other means (than by preparatory conscious work) of influencing our unconscious ? That would be of great importance, in fact not only for invention but also for the whole conduct of life ' —' There is another direction (i.e. other than by preliminary study) in which education of the unconscious could be pursued, though I cannot undertake to speak of it . . . very powerful means for that purpose may be supplied by the methods of psychoanalysis.'

The discussion, in subsequent chapters, of ' the preparation stage ', ' the later conscious work ', ' discovery as a synthesis ' and the use of signs probably take us as far as it is possible to go at this level. The distinction between those who think in words—or definite signs—and those (including most mathematicians) who do not, is clearly brought out and so is the existence of different kinds of mathematical minds ; throughout, all the points are illustrated by convincing examples.

It is, perhaps, in the discussion of ' misunderstandings and failures ' that the author gets nearest to the heart of his subject. ' Between the work of the student who tries to solve a problem in geometry or algebra and a work of invention one can say that there is only a difference of degree, a difference of level, both works being of a similar nature. Now, how does it happen that so many are incapable of that work, incapable of understanding mathematics ? ' It is tempting to try to restate some of the central problems from this angle in more modern terms as follows :

Since all the propositions of pure mathematics are tautologies, why are they not immediately apparent to everyone ? Can there be emotional unconscious resistances at work which normally retard, if they do not prevent, even the apprehension of a known mathematical proposition by a student—let alone the discovery of a hitherto unknown one by a research worker ? If so, how are these resistances sometimes overcome, and in what conditions ? Why do many people, even from their childhood, find learning about numbers and ' variables ' repulsive ? Is the common belief well founded that more girls than boys suffer from this disability ? If so, why ? In any case, how does it arise ? and at what stage of emotional growth ?

As the author says at the outset, ' The subject involves two disciplines, psychology and mathematics, and would require, in order to be treated adequately, that one be both a psychologist and a mathematician '. But M Hadamard has shown that it can be, if not treated adequately, at least discussed usefully and interestingly, by a mathematician who would not claim to be a psychologist but who is singularly free from prejudice. It is to be hoped that a younger man will take up the torch.

HUGH TOWNSHEND

Classics of Biology. By A. Pi Suñer (translated by C. M. Stern). Pitman, London, 1955. Pp. x + 337. 35s.

THIS book consists of a number of extracts from important original contributions to fundamental ideas in biology. These extracts are arranged in chapters, each with an historical introduction by Professor Pi Suñer, and each dealing with basic concepts such as cell theory, heredity, causation and design. Most of the extracts are short, but many are not readily accessible, and for this reason alone such a collection could be valuable. For this reason it is to be regretted that the book does not do justice to the abilities of its author.

It seems certain that the translation is unnecessarily obscure in places ; for example, ' diastase ' is used in place of ' enzyme '. The translator has, in the words of the Preface ' enthusiastically ' re-translated some of the extracts selected by the author, and which were originally quoted by Professor Pi Suñer in Spanish. It would be difficult to refer to the re-translations in a more tactful manner ; undoubtedly they have been rendered with enthusiasm. Obscurities also appear in the author's introductions to the chapters ; his description of mitosis is more difficult to follow than are the quoted extracts from Strasburger and Flemming.

The most interesting chapters are probably XIV (Causation and Design) and XVI (The Whole and its Parts). At the present day it seems that the strict determinism of Cl. Bernard (pp. 248-251) is an over-simplification of the problems facing experimental science. Professor von Bertalanffy has contributed notably to the formulation of some problems facing the determinists, and it should be clear that ' causation ' is not the simple principle which Bernard adopts in the extract mentioned above. The problem of ' causation ' in biology could with advantage have been developed at greater length. Similarly, if the introduction to chapter XVI must deal with matters such as ' Analysis of Reality ' and ' Holism ', then more space should be devoted to it ; as it stands it is quite inadequate.

So brief are the comments and extracts, however, that a detailed discussion of Professor Pi Suñer's views cannot usefully be pursued. The lack of clarity, over-simplification, and unreliability of the translations, make the book neither well suited for undergraduate students nor a source-book for a more advanced reader.

D. F. COLE

OBITUARY

Professor Sir Edmund Whittaker, F.R.S.

By the death on 24th March, 1956, of Professor Sir Edmund Whittaker, F.R.S., at the age of eighty-two, the Philosophy of Science Group has lost one of the most distinguished members of its Editorial Board and British mathematics its Grand Old Man.

Born on 24th October, 1873, and educated at Manchester Grammar School and at Trinity College, Cambridge, of which College he later became a Fellow (1896–1907) and in his old age, to his great joy, an Honorary Fellow, Edmund Taylor Whittaker was a leader of British mathematics, both pure and applied, for more than half a century. In 1905 he was elected F.R.S., and in 1906 he became Royal Astronomer of Ireland. In this post he followed a distinguished line of mathematicians, the greatest of whom was William Rowan Hamilton. In January 1912 he left Dublin to succeed G. Chrystal in the Chair at Edinburgh which he was to grace for over thirty years. During this time he built up one of the most active schools of mathematical research in Great Britain. It would be out of place to recapitulate here his many original achievements as a mathematician. Nor need we appraise his mathematical text-books, at least two of which are probably still almost universally regarded by British mathematicians not merely as necessary reading, but as an essential part of their private libraries. His *magnum opus*, however, which was specifically mentioned when he was awarded the Copley Medal by the Royal Society in 1954, *A History of the Theories of Aether and Electricity*, falls within the fields of study which are the specific concern of our Society and Group. This work originally consisted of a single volume, published in 1910, bearing the title *A History of the Theories of Aether and Electricity, from the age of Descartes to the close of the nineteenth century*. The second (revised) edition, published in 1951, appeared as the first volume of what was eventually intended to be a trilogy. It has come to be regarded as a superb historical survey of classical electricity, magnetism, and radiation-theory. Nevertheless, it was the second volume, published in 1953 under the sub-title *The Modern Theories* 1900–1926, which aroused the excitement of students of the physical sciences everywhere. Innumerable expositions have appeared of relativity and quantum theory, but for breadth of scope and mastery of the original sources as well as for brilliance of writing no other single work in the field can be compared with this. Indeed, it would not be altogether fanciful to regard Whittaker as the Gibbon of the history of these subjects, although he was writing about their rise and not about their fall. For the student of the philosophy of modern physics Whittaker's second volume must be regarded as indispensable, both for reading and for reference. Unfortunately, this volume closes in the middle of the most exciting period of modern theoretical developments, and it is doubtful if the projected third volume, which would have included Dirac's theory of the electron, was more than a fragment at the time of the author's death.

Space does not permit more than the briefest comment on this masterly work—an outstanding exception to the general tendency for major British contributions to the history of the physical sciences to be confined to chemistry—but mention must be made of an important feature which aroused considerable criticism, particularly in America. Whittaker attributed the Special Theory of Relativity primarily to Poincaré and Lorentz, remarking almost as it were *en passant* that in the autumn of 1905 ' Einstein published a paper which set forth the relativity theory of Poincaré and Lorentz with some amplifications, and which attracted much attention '. Although I believe (and I mentioned this to Whittaker in a letter) that this view did not do justice to the *originality* of Einstein's philosophy of time and his scientific method, nevertheless I can understand why as a historian he felt that it was necessary to correct the widespread uncritical acceptance of the view that Einstein's contribution was *unique*. (Indeed, where Einstein was unique—in his formulation of General Relativity—it must now be admitted that he was in the long run much less successful.)

Whittaker will also long be remembered as the foremost exponent, after the death of Eddington, of the latter's *Fundamental Theory*. Not only did he see the manuscript through the press, but he also elucidated many of Eddington's ideas, notably in a paper published in *The Mathematical Gazette* in 1945, in his Eddington Memorial Lecture of 1951 and also in his Tarner Lectures at Cambridge, delivered in 1947 and published in 1949 under the title *From Euclid to Eddington*.

As President of the Royal Society of Edinburgh, from 1939 to 1944, he delivered a notable series of annual addresses which were largely devoted to the philosophy and history of physics and mathematics. (It was immediately after this period that he was knighted.) Particular mention must be made here of his address of 27th October, 1941, on *Some Disputed Questions in the Philosophy of the Physical Sciences*, in which he introduced the useful generic concept of ' postulates of impotence ', e.g. ' it is impossible to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects ', or ' it is impossible to measure precisely the momentum of a particle at the same time as a precise measurement of its position is made '. Whittaker showed how a great part of physical science can be founded on postulates such as these and commented that ' It seems possible that while physics must continue to progress by building on experiments, any branch of it which is in a highly developed state may be exhibited as a set of logical deductions from postulates of impotence, as has already happened in thermodynamics '. I believe that this contribution to the controversy concerning the role of *a priori* methods in science which was at its height at about the time when he delivered this address was the most valuable as well as the most urbane.

Among other published lectures bearing on our subject were the Riddell Lectures delivered at Durham in 1941 on *The Beginning and End of the World*, the Donnellan Lectures delivered at Dublin in 1946 on *Space and Spirit*, and the Herbert Spencer Lecture delivered at Oxford in 1948 on *The Modern Approach to Descartes' Problem : the relation of the mathematical and physical sciences to philosophy*. Finally, I cannot forbear to mention a lecture on Laplace given to a meeting in London of the British Society for the History of Science in 1949, in celebration of the bicentenary of his birth, and published in *The Mathematical Gazette*. Although I was in correspondence with Sir Edmund for many years, this was the only occasion on which I ever had the privilege of hearing him lecture, and I was delighted to find that the charm, lucidity and power of his style were not confined to the printed word. It was an unforgettable experience.

G. J. W.

OBITUARY

Hermann Weyl

PROFESSOR Hermann Weyl died on 8th December 1955, a month after his 70th birthday, in Zürich, where he had made his home when he retired from the Institute of Advanced Study.

After having studied under Hilbert in Göttingen, Professor Weyl taught at the Zürich *Technische Hochschule* from 1913 to 1930. He was then called back to be Hilbert's successor. In 1933, Professor Weyl left Germany as a refugee and went to Princeton, where he remained until his retirement in 1951. He was elected a For. Mem. R.S. in 1936.

Professor Weyl began his work with research into pure mathematics ; the theory of numbers, of integral equations, and of the functions of a complex variable were his first interest. In the 1920's he participated in the discussions on the logical foundations of mathematics which were then at their height. But he also became interested in physics, that is, in relativity theory and quantum mechanics. Professor Weyl was the first to have proposed a ' unified field theory ' in which both the electromagnetic and the gravitational fields are represented through the geometrical structure of space-time. Though he disavowed this theory soon afterwards as not being sufficiently empirical, it would be rash to say that subsequent theories of this kind by other authors have met with more success. It was in this work that Professor Weyl enunciated the principle of *gauge invariance* which has proved itself to be of lasting importance. *Space-Time-Matter*, first published in German in 1918 and in English in 1921, has remained a classic of this approach through affine geometry to general relativity.

From 1923 onwards, Professor Weyl turned his attention to continuous groups and their matrix representation. Here, once more, he pioneered a new way ; his book on *The Theory of Groups and Quantum Mechanics* (1928) joined two subjects which have since remained inseparable.

This is the place to remember Professor Weyl's interest in and contribution to the philosophy of science. He belonged to the older generation of scientists who found it natural and even indispensable to think about the concepts basic to their disciplines ; he was never a mere expert and his classical scholarship, for example, illuminated all he wrote.

Although a pupil and successor of Hilbert's, Professor Weyl was more inclined towards intuitionism than towards formalism. He was, however, well aware of the limitations of both theories, especially, of the ontological pitfalls involved in them. This he expressed by saying ' If we want to speak . . . of numbers as concepts or ideal objects, we must . . . refrain from giving them independent existence ; their being exhausts itself in the functional rôle . . .' .

In the *Philosophy of Mathematics and Natural Science*, first published in 1927, he ranged over the whole field, from mathematical logic to quantum physics and even to biology. The revised and augmented English edition (1949) he introduced with the words ' that its message of the interpenetration of science and philosophical thought is today as timely as ever '. It is a book that has to be read more than once if one wants to appreciate the originality and depth of the ideas contained in it. Let me only mention here Professor Weyl's firm belief that scientific method can be applied successfully also to biological and psychological phenomena. ' It may well be that the sciences concerned have not yet reached the required level. But that this limitation is neither fundamental nor permanent is already shown by psychoanalysis, in my opinion.'

Professor Weyl's last book, *Symmetry* (1952), again bears witness to the great civilisation of its author. There he discusses with equal ease and lucidity the principle of symmetry in the arts and in science. It is, I think, characteristic of his whole outlook that he should have finished his life's work in this manner.

E. H. H.

George Alfred Leon Sarton

Professor George Sarton, who died on 22nd March at Cambridge, Massachusetts, was a scholar of international reputation whose influence is found wherever the history of science is studied. Born at Ghent, Belgium, in 1884, he was educated at the University of Ghent, where he studied philosophy, natural science, and mathematics. The influence of Comte, Tannery and Poincaré turned his interests towards the history and philosophy of Science, and by 1912 he was devoting most of his time to the subject that was to occupy the rest of his working life. His *magnum opus* is the *Introduction to the History of Science* (1927-48), a vast reasoned bibliography, with commentary, in five volumes, but besides this indispensable work he wrote some dozen other books, many of them based on lectures, and several hundreds of papers on various aspects of the history and philosophy of science. He was a great advocate and publicist of his subject, on the organisation of which he has left a permanent mark with the foundation of the journals *Isis* (1912) and *Osiris* (1936) and his activities towards the foundation of the International Academy of the History of Science (1938). With the German invasion of Belgium in 1914 he moved first to England and then to the United States of America, where, after some difficult years, he became a research associate of the Carnegie Institution of Washington. Already an honorary lecturer at Harvard, he was given a special study in the Widener Library, and this became the centre of his work on the *Introduction* and the editing of *Isis*, which has found a permanent home in Cambridge (Mass.). In 1940 he was made Professor of the History of Science at Harvard, becoming Emeritus Professor in 1951. He was awarded the Prix Binoux by the Académie des Sciences of Paris in 1915 and 1935, was president of the Third International Congress of the History of Science in 1934, and president of the International Union of the History of Science from 1950. In 1940 he was created a Knight of the Order of Leopold of Belgium. In 1911 he married in England Eleanor Mabel, the daughter of R. Gervase Elwes, Esq., and until her death in 1952 she was the constant companion and support of all his activities. Their only daughter, Miss May Sarton, is well known in America as a writer.

A. C. C.

Frank Sherwood Taylor

Dr Frank Sherwood Taylor, who died on 5th January, was one of the best known of contemporary historians of science. He was a foundation member of the Philosophy of Science Group. Born in 1897, he was educated at Sherborne School and (after military service in the first war) Lincoln College, Oxford, and later took a Ph.D. from the newly established Department of History and Method of Science at University College, London. After some years as a schoolmaster and a Lecturer in Chemistry at Queen Mary College, London, in 1940 he succeeded Dr Gunther as Curator of the Museum of the History of Science, Oxford. In 1950 he was appointed Director of the Science Museum, South Kensington. A prolific writer and an admirable populariser of science and its history, his earliest scholarly contribution was in the history of alchemy, and this remained his favourite subject. He edited *Ambix* from its foundation in 1937, and published numerous articles there and in other learned journals. Among his books on the history of science perhaps *The Alchemists, Founders of Modern Chemistry* (1949) and *Galileo and the Freedom of Thought* (1938) best show his quality.

A. C. C.

Margaret Macdonald

We regret the death of Margaret Macdonald, a founder member of the Philosophy of Science Group. Dr Macdonald, who was Reader in Philosophy at Bedford College, London, was well known for her lucid and careful contributions to the philosophical journals and, latterly, for her writings on aesthetics. She was Editor of *Analysis* since the war.

H. B. A.

Philosophy of Science Conference

The conference proposed in the last number of this *Journal* will be held on 21-23 September 1956, at Ashburne Hall, Manchester (and not as previously announced). The conference will begin at 7 p.m. on Friday 23 September with dinner, followed by a series of short papers. The Assistant Secretary will be pleased to receive titles of papers (10-15 min.) to be read at this session. During Saturday and Sunday three Symposia will take place:

Saturday morning: The Nature and Scope of Scientific Method
Saturday evening: The Status of Irreversible Processes in Physical Theory
Sunday morning: The Rôle of Historical Explanation in Science

Accommodation is available at Ashburne Hall at an approximate cost of 25s. including meals. Admission to the conference will cost 10s. including morning coffee and afternoon tea. Forms of application and other particulars are obtainable from the Assistant Secretary, Mr. R. F. J. Withers, Dept. of Biology, Middlesex Hospital Medical School, London, W.1.

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Andrewartha, H. G. and Birch, L. C., *The Distribution and Abundance of Animals*, Cambridge University Press, pp. xv + 782, £5 12s. 6d.

Beth, E. W., *L'Existence en mathématiques*, Gauthier-Villars, Paris, 1956, pp. 60, 30 fr.

Boirel, René, *Science et technique*, Editions du Griffon, Neuchatel, Suisse, 1955, pp. 116, Fr. 6.80

Braithwaite, R. B., *Theory of Games as a Tool for the Moral Philosopher*, Cambridge University Press, 1955, pp. 76, 6s.

Bridgman, P. W., *Reflections of a Physicist*, Philosophical Library, New York, 1955, pp. xiv + 576, $6.00

Broda, Engelbert, *Ludwig Boltzmann*, Verlag Franz Deuticke, Wien, 1956, pp. viii + 152, DM. 9.50

Brouwer, L. E. J., Beth, E. W. and Heyting, A., *Mathematical Interpretation of Formal Systems*, North-Holland Publishing Co., Amsterdam, 1955, pp. viii + 113, Fl. 12

Büchler, Luciano, *L'armonia dei contrari*, I.G.O.P.P. Trieste, 1955, pp. 125

Colby, Kenneth Mark, *Energy and Structure in Psychoanalysis*, The Ronald Press, New York, 1955, pp. ix + 154, $4.50

Davidson, Donald, Siegel, Sidney and Suppes, Patrick, *Some Experiments and Related Theory on the Measurement of Utility and subjective Probability*, Report No. 4, Standard Value Theory Project & Dept. of Philosophy, Stanford University, California, 1955, pp. 97

Dell'oro, Angiolo Maros, *La teoria fisica*, Cedam, Padova, 1955, pp. 155

Février, Paulette, *L'interprétation physique de la mécanique ondulatoire et des théories quantiques*, Gauthier-Villars, Paris, 1946, pp. viii + 216, 3,200 fr.

Figueroa, Miguel Herrera, *Psicología y criminología*, Richardet, Buenos Aires, 1956, pp. 145

Fourier, Joseph, *The Analytical Theory of Heat*, Dover Publications, New York, 1955, pp. xxxiii + 466, $1.95

Hartmann, Nicolai, *Kleinere Schriften*, Walter de Gruyter, Berlin, 1955, pp. viii + 318, DM. 26

Hawton, Hector (Ed.), *The Rationalist Annual*, Watts, London, 1956, pp. 96, 3s. 6d.

Hutten, Ernest H., *The Language of Modern Physics*, George Allen & Unwin, London, 1956, pp. 278, 21s.

International Congress, *Proceedings of the Second International Congress of the International Union for the Philosophy of Science* (Zurich 1954), 5 volumes, Editions du Griffon, Neuchatel, Suisse, 1955, Fr. 38

International Federation of Modern Literature and Science, Proceedings of the Sixth Triennial Congress, Oxford, 1954, Languages and Literature, Basil Blackwell, Oxford, 1955, pp. xiii + 33, 45s.

Jordan, Pascual, *Science and the Course of History*, Yale University Press, 1955, pp. x + 139, 20s.

Jung, C. G. and Pauli, W., *The Interpretation of Nature and the Psyche* (translated by R. F. C. Hull), Routledge & Kegan Paul, London, 1955, pp. viii + 247, 16s.

Kapp, Reginald O., *Facts and Faith : The Dual Nature of Reality*, Oxford University Press, 1955, pp. 63, 5s

Lins, Mario, *Logico-Semantical Forms of Philosophical Inquiry*, reprinted from *Archivio di filosofia*, No. 3, 1955, Rio de Janeiro, pp. 35

Lins, Mario, *Search for the Functional Invariants of Law*, Rio de Janeiro, 1955, pp. 32

Marsden, Dora, *The Philosophy of Time*, The Holywell Press, Oxford, 1955, pp. 34, 5s.

Maritain, Jacques, *Bergsonian Philosophy and Thomism*, Philosophical Library, New York, 1955, pp. 383, $6.00

Martin, Gottfried, *Kant's Metaphysics and Theory of Science* (translated by P. G. Lucas), Manchester University Press, 1955, pp. viii + 218, 25s.

Mathis, Leland, *Possible Developments in Materialism*, Leland Mathis, Illinois, 1955, pp. 40, $1.0

Menger, Karl, *Calculus : A Modern Approach*, Ginn, New York, 1955, pp. ix + 354

Murti, T. R. V., *The Central Philosophy of Buddhism*, George Allen & Unwin, London, 1955, pp. xiii + 372, 30s.

Nuffield Foundation Tenth Report, The University Press, Oxford, 1955, pp. 125

Pauli, W. (Ed.), with the assistance of L. Rosenfeld and V. Weisskopt, *Niels Bohr and the Development of Physics*, Pergamon Press, London, 1955, pp. vii + 195, 30s.

Pike, Kenneth L., *Language in Relation to a Unified Theory of the Structure of Human Behaviour*, Summer Institute of Linguistics, California, 1955, pp. v + 85

Quastler, Henry (Ed.), *Information Theory in Psychology : Problems and Methods*, The Free Press, Illinois, 1956, pp. x + 436, $6.00

Rossi, Mario M., *A Plea for Man*, Edinburgh University Press, 1956, pp. viii + 167, 9s. 6d.

Runes, Dagobert D., *On the Nature of Man*, Philosophical Library, New York, 1956, pp. 105, $3.00

Samuel, Otto, *Die Ontologie der Kultur*, Walter de Gruyter, Berlin, 1956, pp. vii + 271, DM. 32

Sarton, George, *The Appreciation of Ancient and Medieval Science during the Renaissance* (1450–1600), University of Pennsylvania Press, 1955, pp. xvii + 233, 40s.

Sartre, Jean-Paul, *Existential Psycho-Analysis*, Philosophical Library, New York, 1953, pp. viii + 275, $4.75

Schrödinger, Erwin, *Expanding Universes*, Cambridge University Press, 1956, pp. viii + 93, 17s. 6d.

Speiser, Andreas, *Die geistige Arbeit*, Birkhäuser Verlag, Basel und Stuttgart, 1956, pp. 207, Fr. 19.25

Tomlin, E. W. F., *Living and Knowing*, Faber & Faber, London, 1955, pp. 285, 25s.

Valpola, Veli, *Acta Philosophica Fennica*, Societas Philosophica, Helsinki, 1955, pp. 247

Vigier, Jean-Pierre, *Structure des micro-objets dans l'interprétation causale de la théorie des quanta*, Gauthier-Villars, Paris, 1956, pp. 192, 3,000 fr.

Warren, Gretchen, *Two Essays*, Basil Blackwell, Oxford, 1956, pp. xiv+58, 7s. 6d.

Wedberg, Anders, *Plato's Philosophy of Mathematics*, Almquist & Wiksell, Sweden, 1955, pp. 154, Sw. Krs. 19

Weisskopf, Walter A., *The Psychology of Economics*, Routledge & Kegan Paul, London, 1955, pp. viii + 266, 25s.

Zeuthen, F., *Economic Theory and Method*, Longmans, London, 1955, pp. xii + 364, 30s.

(*b*) ARTICLES

L. Apostel, 'L'unité de la science dans l'oeuvre de Carnap', *Rivista critica di storia della filosofia*, 1955, **10**, 358

Gonzalo Aguirre Beltran, 'Theoría de los Centros Coordinadores', *Seminario de problemas científicos y filosóficos*, 1955, **3**, 1

E. W. Beth, 'Die Stellung der Logik im Gebäude der heutigen Wissenschaft', *Studium Generale*, 1955, **8**, 425

H. Blumenberg, 'Der kopernikanische Umsturz und die Weltstellung des Menschen. Eine Studie zum Zusammenhang von Naturwissenschaft und Geistesgeschichte', *Studium Generale*, 1955, **8**, 637

G. Burniston Brown, 'A Theory of Action-at-a-Distance', *Proceedings of the Physical Society*, 1955, **68**, 672

Enrique Cabrera, 'Consideraciones en torno al principio de contradicción', *Seminario de problemas científicos y filosóficos*, 1955, **6**, 1

'Commemoration of the 300th Birthday of Bernard Nieuwentijt', *Synthese*, 1955, **9**, 147

L. De Broglie, 'Déduction et induction dans la recherche scientifique', *Scientia*, 1955, **90**, 147

C. T. K. Chari, 'Special Relativity and the Lorentz Transformation', (*British*) *Mathematical Gazette*, 1951, **313**, 184

C. T. K. Chari, 'Zeno's Paradoxes Reconsidered', *The Journal of Parapsychology*, 1955, **3**, 153

C. T. K. Chari, 'Parapsychology and the Concept of "Randomness"', *The Journal of Parapsychology*, 1955, **3**, 190

L. Geymonat, 'Riflessioni sulle ricerche di Carnap intorno alla probabilità e all'induzione', *Rivista critica di storia della filosofia*, 1955, **10**, 450

L. Gratton (Eva Perón), 'Punti di vista moderni del problema cosmogonico', *Scientia*, 1955, **90**, 115

L. Gratton (Eva Péron), 'Punti di vista moderni del problema cosmogonico', (Parte Seconda), *Scientia*, 1955, **90**, 150

Adolf Grünbaum, 'Logical and Philosophical Foundations of the Special Theory of Relativity', *American Journal of Physics*, 1955, **23**, 450

Adolf Grünbaum, 'Time and Entropy', *American Scientist*, 1955, **43**, 550

Adolf Grünbaum, 'Modern Science and Refutation of the Paradoxes of Zeno', *The Scientific Monthly*, 1955, **81**, 234

O. Hannibal, 'Das Zeiterleben in der Schizophrenie', *Studium Generale*, 1955, **8**, 607

G. Hardin, 'Meaninglessness of the Word Protoplasm', *The Scientific Monthly*, 1956, **82**, 112

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Mary B. Hesse, 'Action at a Distance in Classical Physics', *Isis*, 1955, **46**, 337

Eugene C. Holmes, 'The Main Philosophical Considerations of Space and Time', *American Journal of Physics*, 1950, **18**, 560

Kazimierz Lesniak, 'Filodemosa traktat o indukcji' (Philodemos' Treatise on Induction (Summary)), *Studia Logica*, 1955, **2**, 77

Horatio Labastida, 'Experiencia y deducción', *Seminario de problemas científicos y filosóficos*, 1955, **7**, 1

J. L. Mackie, Brian Ellis, J. L. Mackie, 'Has the Universe a Beginning in Time?', *Australasian Journal of Philosophy*, 1955, **33**, 118

Paul E. Meehl and Michael Scriven, 'Compatibility of Science and ESP', *Science*, 1956, **123**, 14

E. Minkowski, 'Zum Problem der erlebten Zeit', *Studium Generale*, 1955, **8**, 601

K. Napp-Zinn, 'Interpretation von Versuchs und Beobachtungsergebnissen in der Biologie', *Studium Generale*, 1956, **9**, 31

D. J. O'Connor, 'Causal Statements', *The Philosophical Quarterly*, 1956, **22**, 17

W. Perpeet, 'Was ist Zeit?', *Studium Generale*, 1955, **8**, 531

Andrew G. Pikler, 'The Diatonic Foundation of Hearing', *Acta Psychologica*, 1955, **3**, 432

Michael Polanyi, 'From Copernicus to Einstein', *Encounter*, 1955

K. R. Popper, 'The Arrow of Time', *Nature*, 1956, **177**, 538

K. R. Popper, 'Three Views concerning Human Knowledge,' *Contemporary British Philosophy*, 1956, Vol. III

G. Preti, 'Il problema della L-verità nella semantica carnapiana', *Rivista critica di storia della filosofia*, 1955, **10**, 409

E. Rabaud, 'L'adaptation des organismes et la suppression du pire', *Scientia*, 1955, **90**, 162

Samuel Ramos, 'El problema del a priori y la experiencia', *Seminario de problemas científicos y filosóficos*, 1955, **8**, 1

K. W. Rankin, 'Linguistic Analyses and the Justification of Induction', *The Philosophical Quarterly*, 1955, **5**, 316

F. Schneider, 'Philosophie und Fachwissenschaft', *Studium Generale*, 1955, **8**, 432

Richard Taylor, Arturo Rosenblueth and Norbert Wiener, 'Controversia sobre la Intencionalidad del Comportamiento', *Suplementos del seminario de problemas científicos y filosóficos*, 1955, **2**, 1

John Todd and Kenneth Dewhurst, 'The Double; Its Psycho-Pathology and Psycho-Physiology', *The Journal of Nervous and Mental Disease*, 1955, **1**, 47

A. Wenzl, 'Die Objektivität der Zeit', *Studium Generale*, 1955, **8**, 545

ANNOUNCEMENT

We have noted with pleasure that a Conference on 'Science and the Modern World View—Toward a Common Understanding of the Sciences and the Humanities' took place in May at Cambridge, Mass., in honour of Dr P. W. Bridgman and Dr Philipp Frank, who have retired from active teaching in the University of Harvard. The gathered proceedings, including concise statements of significant parts of the discussion, are to be published in book form.